PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXVIII. For the Year 1788.

PART II.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

MDCCLXXXVIII.

CONTENTS

OF

VOL. LXXVIII. PART II.

XIV. *OBSERVATIONS on the Natural History of the* Cuckoo. *By Mr.* Edward Jenner. *In a Letter to* John Hunter, *Esq. F. R. S.* Page 219

XV. *Of the Temperament of those musical Instruments, in which the Tones, Keys, or Frets, are fixed, as in the Harpsichord, Organ, Guitar, &c. By Mr.* Tiberius Cavallo, *F. R. S.* p. 238

XVI. *Description of a new Electrical Instrument capable of collecting together a diffused or little condensed Quantity of Electricity. By Mr.* Tiberius Cavallo, *F. R. S.* p. 255

XVII. *On the Conversion of a Mixture of dephlogisticated and phlogisticated Air into nitrous Acid, by the electric Spark. By* Henry Cavendish, *Esq. F. R. S. and A. S.* p. 261

XVIII. *Experiments on the Effect of various Substances in lowering the Point of Congelation in Water. By* Charles Blagden, *M. D. Sec. R. S. and F. A. S.* p. 277

XIX. *Additional Experiments and Observations relating to the Principle of Acidity, the Decomposition of Water, and Phlogiston. By* Joseph Priestley, *LL.D. F. R. S. With Letters to him on the Subject, by Dr.* Withering, *and* James Keir, *Esq.* p. 313

XX. *On the Probabilities of Survivorships between Two Persons of any given Ages, and the Method of determining the Values of Reversions depending on those Survivorships. By Mr.* William

liam Morgan; *communicated by the Rev.* Richard Price, *D. D. F. R. S.* p. 331

XXI. *An Account of a remarkable Tranſpoſition of the Viſcera. By* Matthew Baillie, *M. D. In a Letter to* John Hunter, *Eſq. F. R. S.* p. 350

XXII. *On the* Georgian *Planet and its Satellites. By* William Herſchel, *LL.D. F. R. S.* p. 364

XXIII. *Experiments on the Formation of Volatile Alkali, and on the Affinities of the Phlogiſticated and light Inflammable Airs. By* William Auſtin, *M. D. Fellow of the College of Phyſicians; communicated by* Charles Blagden, *M. D. Sec. R. S.* p. 379

XXIV. *Some Properties of the Sum of the Diviſors of Numbers. By* Edward Waring, *M. D. F. R. S.* p. 388

XXV. *Experiments on the Production of artificial Cold. By Mr.* Richard Walker, *Apothecary to the* Radcliffe Infirmary *at* Oxford. *In a Letter* to Henry Cavendiſh, *Eſq. F.R.S. and A.S.* p. 395

XXVI. *A Deſcription of an Inſtrument which, by the turning of a Winch, produces the Two States of Electricity without Friction or Communication with the Earth. In a Letter from Mr.* William Nicholſon *to Sir* Joſeph Banks, *Bart. P. R. S.* p. 403

XXVII. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain at* Lyndon *in* Rutland, *with the Rain in* Hampſhire *and* Surrey, *in* 1787. *Alſo ſome Account of the Annual Growth of Trees. By* Thomas Barker, *Eſq. Communicated by* Thomas White, *Eſq. F.R.S.* p. 408

XXVIII. *On the Era of the Mahometans, called the Hejerà* (هجرة). *By* William Marſden, *Eſq. F. R. S. and A. S.* p. 414

PHI-

PHILOSOPHICAL TRANSACTIONS.

XIV. *Observations on the Natural History of the* Cuckoo. *By Mr.* Edward Jenner. *In a Letter to* John Hunter, *Esq. F. R. S.*

Read March 13, 1788.

TO JOHN HUNTER, ESQ.

DEAR SIR,

HAVING, at your request, employed some of my leisure hours in attending to the natural history of the Cuckoo, I beg leave to lay before you the result of my observations,

with a hope that they may tend to illuſtrate a ſubject hitherto not ſufficiently inveſtigated; and ſhould what is here offered prove, in your opinion, deſerving the attention of the Royal Society, you will do me the honour of preſenting it to that learned Body.

The firſt appearance of Cuckoos in Glouceſterſhire (the part of England where theſe obſervations were made) is about the 17th of April. The ſong of the male, which is well known, ſoon proclaims its arrival. The ſong of the female (if the peculiar notes of which it is compoſed may be ſo called) is widely different, and has been ſo little attended to, that I believe few are acquainted with it. I know not how to convey to you a proper idea of it by a compariſon with the notes of any other bird; but the cry of the Dab-chick bears the neareſt reſemblance to it.

Unlike the generality of birds, Cuckoos do not pair. When a female appears on the wing, ſhe is often attended by two or three males, who ſeem to be earneſtly contending for her favours. From the time of her appearance, till after the middle of ſummer, the neſts of the birds ſelected to receive her egg are to be found in great abundance; but, like the other migrating birds, ſhe does not begin to lay till ſome weeks after her arrival. I never could procure an egg till after the middle of May, though probably an early-coming Cuckoo may produce one ſooner *.

* What is meant by an *early-coming* Cuckoo, I ſhall more fully explain in a Paper I intend to lay before you on the Migration of Birds; but it may be neceſſary to mention here, that migrating birds of the ſame ſpecies arrive and depart in ſucceſſion. Cuckoos, for example, appear in greater numbers on the the ſecond than on the week of their arrival, and they diſappear in the ſame gradual manner.

The

The Cuckoo makes choice of the neſts of a great variety of ſmall birds. I have known its egg intruſted to the care of the Hedge-ſparrow, the Water-wagtail, the Titlark, the Yellow-hammer, the green Linnet, and the Whinchat. Among theſe it generally ſelects the three former; but ſhews a much greater partiality to the Hedge-ſparrow than to any of the reſt: therefore, for the purpoſe of avoiding confuſion, this bird only, in the following account, will be conſidered as the foſter-parent of the Cuckoo, except in inſtances which are particularly ſpecified.

The Hedge-ſparrow commonly takes up four or five days in laying her eggs. During this time (generally after ſhe has laid one or two) the Cuckoo contrives to depoſit her egg among the reſt, leaving the future care of it entirely to the Hedge-ſparrow. This intruſion often occaſions ſome diſcompoſure; for the old Hedge-ſparrow at intervals, whilſt ſhe is ſitting, not unfrequently throws out ſome of her own eggs, and ſometimes injures them in ſuch a way that they become addle; ſo that it more frequently happens, that only two or three Hedge-ſparrow's eggs are hatched with the Cuckoo's than otherwiſe: but whether this be the caſe or not, ſhe ſits the ſame length of time as if no foreign egg had been introduced, the Cuckoo's egg requiring no longer incubation than her own. However, I have never ſeen an inſtance where the Hedge-ſparrow has either thrown out or injured the egg of the Cuckoo.

When the Hedge-ſparrow has ſat her uſual time, and diſengaged the young Cuckoo and ſome of her own offspring from the ſhell *, her own young ones, and any of her eggs that remain unhatched, are ſoon turned out, the young Cuckoo remaining poſſeſſor of the neſt, and ſole object of her future

* The young Cuckoo is commonly hatched firſt.

care. The young birds are not previouſly killed, nor are the eggs demoliſhed; but all are left to periſh together, either entangled about the buſh which contains the neſt, or lying on the ground under it.

The early fate of the young Hedge-ſparrows is a circumſtance that has been noticed by others, but attributed to wrong cauſes. A variety of conjectures have been formed upon it. Some have ſuppoſed the parent Cuckoo the author of their deſtruction; while others, as erroneouſly, have pronounced them ſmothered by the diſproportionate ſize of their fellow-neſtling. Now the Cuckoo's egg being not much larger than the Hedge-ſparrow's (as I ſhall more fully point out hereafter) it neceſſarily follows, that at firſt there can be no great difference in the ſize of the birds juſt burſt from the ſhell. Of the fallacy of the former aſſertion alſo I was ſome years ago convinced, by having found that many Cuckoo's eggs were hatched in the neſts of other birds after the old Cuckoo had diſappeared; and by ſeeing the ſame fate then attend the neſtling ſparrows as during the appearance of old Cuckoos in this country. But, before I proceed to the facts relating to the death of the young Sparrows, it will be proper to lay before you ſome examples of the incubation of the egg, and the rearing of the young Cuckoo; ſince even the well known fact, that this buſineſs is intruſted to the care of other birds, has been controverted by an Author who has lately written on this ſubject*; and ſince, as it is a fact ſo much out of the ordinary courſe of nature, it may ſtill probably be diſbelieved by others.

* The Hon. DAINES BARRINGTON.

EXAMPLE

EXAMPLE I.

The Titlark is frequently ſelected by the Cuckoo to take charge of its young one; but as it is a bird leſs familiar than many that I have mentioned, its neſt is not ſo often diſcovered. I have, nevertheleſs, had ſeveral Cuckoo's eggs brought to me that were found in Titlark's neſts; and had one opportunity of ſeeing the young Cuckoo in the neſt of this bird: I ſaw the old birds feed it repeatedly, and, to ſatisfy myſelf that they were really Titlarks, ſhot them both, and found them to be ſo.

EXAMPLE II.

A Cuckoo laid her egg in a Water-wagtail's neſt in the thatch of an old cottage. The Wagtail ſat her uſual time, and then hatched all the eggs but one; which, with all the young ones, except the Cuckoo, was turned out of the neſt. The young birds, conſiſting of five, were found upon a rafter that projected from under the thatch, and with them was the egg, not in the leaſt injured. On examining the egg, I found the young Wagtail it contained quite perfect, and juſt in ſuch a ſtate as birds are when ready to be diſengaged from the ſhell. The Cuckoo was reared by the Wagtails till it was nearly capable of flying, when it was killed by an accident.

EXAMPLE III.

A Hedge-ſparrow built her neſt in a hawthorn buſh in a timber-yard: after ſhe had laid two eggs, a Cuckoo dropped in a third.

a third. The Sparrow continued laying, as if nothing had happened, till ſhe had laid five, her uſual number, and then ſat.

June 20, 1786. On inſpecting the neſt I found, that the bird had hatched this morning, and that every thing but the young Cuckoo was thrown out. Under the neſt I found one of the young Hedge-ſparrows dead, and one egg by the ſide of the neſt entangled with the coarſe woody materials that formed its outſide covering. On examining the egg, I found one end of the ſhell a little cracked, and could ſee that the Sparrow it contained was yet alive. It was then reſtored to the neſt, but in a few minutes was thrown out. The egg being again ſuſpended by the outſide of the neſt, was ſaved a ſecond time from breaking. To ſee what would happen if the Cuckoo was removed, I took out the Cuckoo, and placed the egg containing the Hedge-ſparrow in the neſt in its ſtead. The old birds, during this time, flew about the ſpot, ſhewing ſigns of great anxiety; but when I withdrew, they quickly came to the neſt again. On looking into it in a quarter of an hour afterwards, I found the young one completely hatched, warm and lively. The Hedge-ſparrows were ſuffered to remain undiſturbed with their new charge for three hours (during which time they paid every attention to it) when the Cuckoo was again put into the neſt. The old Sparrows had been ſo much diſturbed by theſe intruſions, that for ſome time they ſhewed an unwillingneſs to come to it: however, at length they came, and on examining the neſt again in a few minutes, I found the young Sparrow was tumbled out. It was a ſecond time reſtored, but again experienced the ſame fate.

From theſe experiments, and ſuppoſing, from the feeble appearance of the young Cuckoo juſt diſengaged from the ſhell,

that it was utterly incapable of diſplacing either the egg or the young Sparrows, I was induced to believe, that the old Sparrows were the only agents in this ſeeming unnatural buſineſs; but I afterwards clearly perceived the cauſe of this ſtrange phænomenon, by diſcovering the young Cuckoo in the act of diſplacing its fellow-neſtlings, as the following relation will fully evince.

June 18, 1787, I examined the neſt of a Hedge-ſparrow, which then contained a Cuckoo's and three Hedge-ſparrow's eggs. On inſpecting it the day following, I found the bird had hatched, but that the neſt now contained only a young Cuckoo and one young Hedge-ſparrow. The neſt was placed ſo near the extremity of a hedge, that I could diſtinctly ſee what was going forward in it; and, to my aſtoniſhment, ſaw the young Cuckoo, though ſo newly hatched, in the act of turning out the young Hedge-ſparrow.

The mode of accompliſhing this was very curious. The little animal, with the aſſiſtance of its rump and wings, contrived to get the bird upon its back, and making a lodgement for the burden by elevating its elbows, clambered backward with it up the ſide of the neſt till it reached the top, where reſting for a moment, it threw off its load with a jerk, and quite diſengaged it from the neſt. It remained in this ſituation a ſhort time, feeling about with the extremities of its wings, as if to be convinced whether the buſineſs was properly executed, and then dropped into the neſt again. With theſe (the extremities of its wings) I have often ſeen it examine, as it were, an egg and neſtling before it began its operations; and the nice ſenſibility which theſe parts appeared to poſſeſs ſeemed ſufficiently to compenſate the want of ſight, which as yet it was deſtitute of. I afterwards put in an egg, and this, by a ſimilar

ſimilar proceſs, was conveyed to the edge of the neſt, and thrown out. Theſe experiments I have ſince repeated ſeveral times in different neſts, and have always found the young Cuckoo diſpoſed to act in the ſame manner. In climbing up the neſt, it ſometimes drops its burden, and thus is foiled in its endeavours; but, after a little reſpite, the work is reſumed, and goes on almoſt inceſſantly till it is effected. It is wonderful to ſee the extraordinary exertions of the young Cuckoo, when it is two or three days old, if a bird be put into the neſt with it that is too weighty for it to lift out. In this ſtate it ſeems ever reſtleſs and uneaſy. But this diſpoſition for turning out its companions begins to decline from the time it is two or three till it is about twelve days old, when, as far as I have hitherto ſeen, it ceaſes. Indeed, the diſpoſition for throwing out the egg appears to ceaſe a few days ſooner; for I have frequently ſeen the young Cuckoo, after it had been hatched nine or ten days, remove a neſtling that had been placed in the neſt with it, when it ſuffered an egg, put there at the ſame time, to remain unmoleſted. The ſingularity of its ſhape is well adapted to theſe purpoſes; for, different from other newly-hatched birds, its back from the *ſcapulæ* downwards is very broad, with a conſiderable depreſſion in the middle. This depreſſion ſeems formed by nature for the deſign of giving a more ſecure lodgement to the egg of the Hedge-ſparrow, or its young one, when the young Cuckoo is employed in removing either of them from the neſt. When it is about twelve days old, this cavity is quite filled up, and then the back aſſumes the ſhape of neſtling birds in general.

Having found that the old Hedge-ſparrow commonly throws out ſome of her own eggs after her neſt has received the Cuckoo's, and not knowing how ſhe might treat her young

ones if the young Cuckoo was deprived of the power of diſpoſſeſſing them of the neſt, I made the following experiment.

July 9. A young Cuckoo, that had been hatched by a Hedge-ſparrow about four hours, was confined in the neſt in ſuch a manner that it could not poſſibly turn out the young Hedge-ſparrows which were hatched at the ſame time, though it was almoſt inceſſantly making attempts to effect it. The conſequence was, the old birds fed the whole alike, and appeared in every reſpect to pay the ſame attention to their own young as to the young Cuckoo, until the 13th, when the neſt was unfortunately plundered.

The ſmallneſs of the Cuckoo's egg in proportion to the ſize of the bird is a circumſtance that hitherto, I believe, has eſcaped the notice of the ornithologiſt. So great is the diſproportion, that it is in general ſmaller than that of the Houſe-ſparrow; whereas the difference in the ſize of the birds is nearly as five to one. I have uſed the term *in general*, becauſe eggs produced at different times by the ſame bird vary very much in ſize. I have found a Cuckoo's egg ſo light that it weighed only forty-three grains, and one ſo heavy that it weighed fifty-five grains. The colour of the Cuckoo's eggs is extremely variable. Some, both in ground and penciling, very much reſemble the Houſe-ſparrow's; ſome are indiſtinctly covered with bran-coloured ſpots; and others are marked with lines of black, reſembling, in ſome meaſure, the eggs of the Yellow-hammer.

The circumſtance of the young Cuckoo's being deſtined by nature to throw out the young Hedge-ſparrows, ſeems to account for the parent-cuckoo's dropping her egg in the neſts of birds ſo ſmall as thoſe I have particulariſed. If ſhe were to do this in the neſt of a bird which produced a large egg, and conſequently a large neſtling, the young Cuckoo would pro-

bably find an inſurmountable difficulty in ſolely poſſeſſing the neſt, as its exertions would be unequal to the labour of turning out the young birds*. Beſides, though many of the larger birds might have fed the neſtling Cuckoo very properly, had it been committed to their charge, yet they could not have ſuffered their own young to have been ſacrificed, for the accommodation of the Cuckoo, in ſuch great number as the ſmaller ones, which are ſo much more abundant; for though it would be a vain attempt to calculate the numbers of neſtlings deſtroyed by means of the Cuckoo, yet the ſlighteſt obſervation would be ſufficient to convince us that they muſt be very large.

Here it may be remarked, that though nature permits the young Cuckoo to make this great waſte, yet the animals thus deſtroyed are not thrown away or rendered uſeleſs. At the ſeaſon when this happens, great numbers of tender quadrupeds and reptiles are ſeeking proviſion; and if they find the callow neſtlings which have fallen victims to the young Cuckoo, they are furniſhed with food well adapted to their peculiar ſtate.

It appears a little extraordinary, that two Cuckoo's eggs ſhould ever be depoſited in the ſame neſt, as the young one produced from one of them muſt inevitably periſh; yet I have known two inſtances of this kind, one of which I ſhall relate.

* I have known an inſtance in which a Hedge-ſparrow ſat upon a Cuckoo's egg and one of her own. Her own egg was hatched five days before the Cuckoo's, when the young Hedge-ſparrow had gained ſuch a ſuperiority in ſize that the young Cuckoo had not powers ſufficient to lift it out of the neſt till it was two days old, by which time it was grown very conſiderably. This egg was probably laid by the Cuckoo ſeveral days after the Hedge-ſparrow had begun to ſit; and even in this caſe it appears, that its preſence had created the diſturbance before alluded to, as all the Hedge-ſparrow's eggs were gone except one.

June 27, 1787. Two Cuckoos and a Hedge-ſparrow were hatched in the ſame neſt this morning; one Hedge-ſparrow's egg remained unhatched. In a few hours after, a conteſt began between the Cuckoos for the poſſeſſion of the neſt, which continued undetermined till the next afternoon; when one of them, which was ſomewhat ſuperior in ſize, turned out the other, together with the young Hedge-ſparrow and the unhatched egg. This conteſt was very remarkable. The combatants alternately appeared to have the advantage, as each carried the other ſeveral times nearly to the top of the neſt, and then ſunk down again, oppreſſed by the weight of its burden; till at length, after various efforts, the ſtrongeſt prevailed, and was afterwards brought up by the Hedge-ſparrows.

I come now, Sir, to conſider the principal matter that has agitated the mind of the naturaliſt reſpecting the Cuckoo: *why, like other birds, it ſhould not build a neſt, incubate its eggs, and rear its own young?*

There is certainly no reaſon to be aſſigned from the formation of this bird why, in common with others, it ſhould not perform all theſe ſeveral offices; for it is in every reſpect perfectly formed for collecting materials and building a neſt. Neither its external ſhape nor internal ſtructure prevent it from incubation; nor is it by any means incapacitated from bringing food to its young. It would be needleſs to enumerate the various opinions of authors on this ſubject from ARISTOTLE to the preſent time. Thoſe of the ancients appear to be either viſionary, or erroneous; and the attempts of the moderns towards its inveſtigation have been confined within very narrow limits; for they have gone but little farther in their reſearches than to examine the conſtitution and ſtructure of the bird, and

having found it poſſeſſed of a capacious ſtomach with a thin external covering, concluded that the preſſure upon this part, in a ſitting poſture, prevented incubation. They have not conſidered that many of the birds which incubate have ſtomachs analogous to thoſe of Cuckoos: the ſtomach of the Owl, for example, is proportionably capacious, and is almoſt as thinly covered with external integuments. Nor have they conſidered, that the ſtomachs of neſtlings are always much diſtended with food; and that this very part, during the whole time of their confinement to the neſt, ſupports, in a great degree, the weight of the whole body; whereas, in a ſitting bird, it is not nearly ſo much preſſed upon; for the breaſt in that caſe fills up chiefly the cavity of the neſt, for which purpoſe, from its natural convexity, it is admirably well fitted.

Theſe obſervations, I preſume, may be ſufficient to ſhew that the Cuckoo is not rendered incapable of ſitting through a peculiarity either in the ſituation or formation of the ſtomach; yet, as a proof ſtill more deciſive, I ſhall lay before you the following fact.

In the ſummer of the year 1786, I ſaw, in the neſt of a Hedge-ſparrow, a Cuckoo, which, from its ſize and plumage, appeared to be nearly a fortnight old. On lifting it up in the neſt, I obſerved two Hedge-ſparrow's eggs under it. At firſt I ſuppoſed them part of the number which had been ſat upon by the Hedge-ſparrow with the Cuckoo's egg, and that they had become addle, as birds frequently ſuffer ſuch eggs to remain in their neſts with their young; but on breaking one of them I found it contained a living fœtus; ſo that of courſe theſe eggs muſt have been laid ſeveral days after the Cuckoo was hatched, as the latter now completely filled up the neſt, and was by

this peculiar incident performing the part of a ſitting-bird *.

Having under my inſpection, in another Hedge-ſparrow's neſt, a young Cuckoo, about the ſame ſize as the former, I procured two Wagtail's eggs which had been ſat upon a few days, and had them immediately conveyed to the ſpot, and placed under the Cuckoo. On the ninth day after the eggs had been in this ſituation, the perſon appointed to ſuperintend the neſt (as it was ſome diſtance from the place of my reſidence) came to inform me, that the Wagtails were hatched. On going to the place, and examining the neſt, I found nothing in it but the Cuckoo and the ſhells of the Wagtail's eggs. The fact, therefore, of the birds being hatched, I do not give you as coming immediately under my own eye; but the teſtimony of the perſon appointed to watch the neſt was corroborated by that of another witneſs.

To what cauſe then may we attribute the ſingularities of the Cuckoo? May they not be owing to the following circumſtances? *The ſhort reſidence this bird is allowed to make in the country where it is deſtined to propagate its ſpecies, and the call that nature has upon it, during that ſhort reſidence, to produce a numerous progeny.* The Cuckoo's firſt appearance here is about the middle of April, commonly on the 17th. Its egg is not ready for incubation till ſome weeks after its arrival, ſeldom before the middle of May. A fortnight is taken up by the ſitting bird in hatching the egg. The young bird generally continues three weeks in the neſt before it flies, and the

* At this time I was unacquainted with the fact, that the young Cuckoo turned out the eggs of the Hedge-ſparrow; but it is reaſonable to conclude, that it had loſt the diſpoſition for doing this when theſe eggs were depoſited in the neſt.

foſter--

foſter-parents feed it more than five weeks after this period; ſo that, if a Cuckoo ſhould be ready with an egg much ſooner than the time pointed out, not a ſingle neſtling, even one of the earlieſt, would be fit to provide for itſelf before its parent would be inſtinctively directed to ſeek a new reſidence, and be thus compelled to abandon its young one; for old Cuckoos take their final leave of this country the firſt week in July.

Had nature allowed the Cuckoo to have ſtaid here as long as ſome other migrating birds, which produce a ſingle ſet of young ones (as the Swift or Nightingale, for example), and had allowed her to have reared as large a number as any bird is capable of bringing up at one time, theſe might not have been ſufficient to have anſwered her purpoſe; but by ſending the Cuckoo from one neſt to another, ſhe is reduced to the ſame ſtate as the bird whoſe neſt we daily rob of an egg, in which caſe the ſtimulus for incubation is ſuſpended. Of this we have a familiar example in the common domeſtic fowl. That the Cuckoo actually lays a great number of eggs, diſſection ſeems to prove very deciſively. Upon a compariſon I had an opportunity of making between the ovarium, or racemus vitellorum, of a female Cuckoo, killed juſt as ſhe had begun to lay, and of a pullet killed in the ſame ſtate, no eſſential difference appeared. The uterus of each contained an egg perfectly formed, and ready for excluſion; and the ovarium exhibited a large cluſter of eggs gradually advanced from a very diminutive ſize, to the greateſt the yolk acquires before it is received into the oviduct. The appearance of one killed on the third of July was very different. In this I could diſtinctly trace a great number of the membranes which had diſcharged yolks into the oviduct; and one of them appeared as if it had parted with a yolk the preceding day. The ovarium ſtill exhibited a cluſter

cluſter of enlarged eggs; but the moſt forward of them was ſcarcely larger than a muſtard-ſeed.

I would not be underſtood, Sir, to advance that every egg which ſwells in the ovarium at the approach or commencement of the propagating ſeaſon is brought to perfection; but it appears clearly, that a bird, in obedience to the dictates of her own will, or to ſome hidden cauſe in the animal œconomy, can either retard or bring forward her eggs. Beſides the example of the common fowl above alluded to, many others occur. If you deſtroy the neſt of a Blackbird, a Robin, or almoſt any ſmall bird, in the ſpring, when ſhe has laid her uſual number of eggs, it is well known to every one, who has paid any attention to enquiries of this kind, in how ſhort a ſpace of time ſhe will produce a freſh ſet. Now, had the bird been ſuffered to have proceeded without interruption in her natural courſe, the eggs would have been hatched, and the young ones brought to a ſtate capable of providing for themſelves, before ſhe would have been induced to make another neſt, and excited to produce another ſet of eggs from the ovarium. If the bird had been deſtroyed at the time ſhe was ſitting on her firſt laying of eggs, diſſection would have ſhewn the ovarium containing a great number in an enlarged ſtate, and advancing in the uſual progreſſive order. Hence it plainly appears, that birds can keep back or bring forward (under certain limitations) their eggs at any time during the ſeaſon appointed for them to lay; but the Cuckoo, not being ſubject to the common interruptions, goes on laying from the time ſhe begins, till the eve of her departure from this country: for although old Cuckoos in general take their leave the firſt week in July (and I never could ſee one after the 5th day of that

month *), yet I have known an inſtance of an egg's being hatched in the neſt of a Hedge-ſparrow ſo late as the 15th. And a farther proof of their continuing to lay till the time of their leaving us may, I think, be fairly deduced from the appearances on diſſection of the female Cuckoo above-mentioned, killed on the 3d of July.

Among the many peculiarities of the young Cuckoo, there is one that ſhews itſelf very early. Long before it leaves the neſt, it frequently, when irritated, aſſumes the manner of a bird of prey, looks ferocious, throws itſelf back, and pecks at any thing preſented to it with great vehemence, often at the ſame time making a chuckling noiſe like a young hawk. Sometimes, when diſturbed in a ſmaller degree, it makes a kind of hiſſing noiſe, accompanied with a heaving motion of the whole body †. The growth of the young Cuckoo is uncommonly rapid.

The chirp is plaintive, like that of the Hedge-ſparrow; but the ſound is not acquired from the foſter-parent, as it is the ſame whether it be reared by the Hedge-ſparrow, or any other bird.

It never acquires the adult note during its ſtay in this country.

The ſtomachs of young Cuckoos contain a great variety of food. On diſſecting one that was brought up by Wagtails,

* Though I am unacquainted with an inſtance, yet I conceive it poſſible, that here and there a ſtraggling Cuckoo may be ſeen after this time.

† Young animals, being deprived of other modes of defence, are probably endowed with the powers of exciting fear in their common enemies. If you but ſlightly touch the young Hedge-hog, for inſtance, before it becomes fully armed with its prickly coat, the little animal jumps up with a ſudden ſpring, and imitates very cloſely the ſound of the word *huſh!* as we pronounce it in a loud whiſper. This diſpoſition is apparent in many other animals.

and

and fed by them at the time it was ſhot (though it was nearly of the ſize and fulneſs of plumage of the parent-bird) I found in its ſtomach the following ſubſtances.

Flies and Beetles of various kinds.

Small Snails, with their ſhells unbroken.

Graſhoppers.

Caterpillars.

Part of a Horſe-bean.

A vegetable ſubſtance reſembling bits of tough graſs, rolled into a ball.

The ſeeds of a vegetable that reſembled thoſe of the gooſe-graſs.

In the ſtomach of one fed by Hedge-ſparrows, the contents were almoſt entirely vegetable; ſuch as wheat, ſmall vetches, &c. But this was the only inſtance of the kind I had ever ſeen, as theſe birds, in general, feed the young Cuckoo with ſcarcely any thing but animal food. However, it ſerved to clear up a point which before had ſomewhat puzzled me; for having found the Cuckoo's egg in the neſt of a green linnet, which begins very early to feed its young with vegetable food, I was apprehenſive, till I ſaw this fact, that this bird would have been an unfit foſter-parent for the young Cuckoo.

The Titlark, I obſerve, feeds it principally with Graſhoppers.

But the moſt ſingular ſubſtance, ſo often met with in the ſtomachs of young Cuckoos, is a ball of hair curiouſly wound up. I have found it of various ſizes, from that of a pea to that of a ſmall nutmeg. It ſeems to be compoſed chiefly of Horſe-hairs, and from the reſemblance it bears to the inſide covering of the neſt, I conceive the bird ſwallows it while a neſtling. In the ſtomachs of old Cuckoos I have often ſeen

maſſes of hair; but theſe had evidently once formed a part of the hairy Caterpillar, which the Cuckoo often takes for its food.

There ſeems to be no preciſe time fixed for the departure of young Cuckoos. I believe they go off in ſucceſſion, probably as ſoon as they are capable of taking care of themſelves; for although they ſtay here till they become nearly equal in ſize and growth of plumage to the old Cuckoo, yet in this very ſtate the foſtering care of the Hedge-ſparrow is not withdrawn from them. I have frequently ſeen the young Cuckoo of ſuch a ſize that the Hedge-ſparrow has perched on its back, or half-expanded wing, in order to gain ſufficient elevation to put the food into its mouth. At this advanced ſtage, I believe that young Cuckoos procure ſome food for themſelves; like the young rook, for inſtance, which in part feeds itſelf, and is partly fed by the old ones till the approach of the pairing ſeaſon. If they did not go off in ſucceſſion, it is probable we ſhould ſee them in large numbers by the middle of Auguſt; for as they are to be found in great plenty *, when in a neſtling ſtate, they muſt now appear very numerous, ſince all of them muſt have quitted the neſt before this time. But this is not the caſe; for they are not more numerous at any ſeaſon than the parent birds are in the months of May and June.

The ſame inſtinctive impulſe which directs the Cuckoo to depoſit her eggs in the neſts of other birds, directs her young one to throw out the eggs and young of the owner of the neſt. The ſcheme of nature would be incomplete without it; for it would be extremely difficult, if not impoſſible, for the little birds, deſtined to find ſuccour for the Cuckoo, to find it alſo

* I have known four young Cuckoos in the neſts of Hedge-ſparrows in a ſmall paddock at the ſame time.

for

for their own young ones, after a certain period; nor would there be room for the whole to inhabit the neſt.

Thus, Sir, I have, with much pleaſure, complied with your requeſt; and here lay before you ſuch obſervations as I have hitherto been capable of making on the natural hiſtory of the Cuckoo; and ſhould they throw ſome light on a ſubject that has long lain hid in obſcurity, I ſhall not think my time has been ill employed.

With a grateful ſenſe of the many obligations I owe to the friendſhip with which you have ſo long honoured me,

I remain, &c.

EDW. JENNER.

Berkeley,
December 27, 1787.

XV. *Of the Temperament of thoſe muſical Inſtruments, in which the Tones, Keys, or Frets, are fixed, as in the Harpſichord, Organ, Guitar, &c.* By *Mr.* Tiberius Cavallo, *F.R.S.*

Read April 3, 1788.

THE ſcale of muſic, which is uſed at preſent, conſiſts of ſeven principal notes or ſounds, which muſicians denote by the letters of the alphabet A, B, C, D, E, F, and G; which, together with ſome intermediate ones, commonly called flats and ſharps, and the octave of the firſt, make 13 ſounds.

When thoſe ſounds are conſidered with reſpect to the firſt, they are called by the following names, *viz.* the prime or key-note, the ſecond minor, ſecond, third minor, third major, fourth, fourth major, fifth, ſixth minor, ſixth major, ſeventh minor, ſeventh major, and octave.

Muſical ſounds are produced by the vibrations of the ſonorous bodies, and they are acuter or graver as the vibrations performed in a given time are more or leſs in number; ſo that if a ſtring vibrating 100 times in a ſecond produces a certain ſound, and another ſtring vibrating 120 times in a ſecond produces another ſound, the latter is ſaid to be acuter, higher, or ſharper than the former.

The number of vibrations performed in a certain time principally depends on the thickneſs, length, and elaſticity of the ſonorous bodies; but as the ſimpleſt ſonorous bodies, and the fitteſt for examination, are thoſe ſtrings which are equal in every

other reſpect, excepting in their lengths, becauſe the number of vibrations, which they perform in a given time, is ſimply in the proportion of their lengths, we ſhall conſider only thoſe in the preſent inveſtigation, the number of vibrations performed by other ſorts of ſonorous bodies being eaſily deduced from them.

As the above-mentioned 13 ſounds are all different from each other, the ſtrings which produce them differ in length, and of courſe in the number of the vibrations, which, when ſtruck, they perform in a certain time. Here follow the proportions which the numbers of vibrations performed in a given time, or the length of the ſtrings which expreſs thoſe 13 ſounds, bear to the firſt, prime, or key-note.

Firſt	1	Fourth	$\frac{3}{4}$	Seventh minor	$\frac{9}{16}$
Second minor	$\frac{15}{16}$	Fourth major	$\frac{32}{45}$	Seventh major	$\frac{8}{15}$
Second	$\frac{8}{9}$	Fifth	$\frac{2}{3}$	Octave	$\frac{1}{2}$
Third minor	$\frac{5}{6}$	Sixth minor	$\frac{5}{8}$		
Third major	$\frac{4}{5}$	Sixth major	$\frac{3}{5}$		

If, inſtead of many ſtrings having thoſe lengths in order to expreſs the 13 ſounds, or notes of an octave, one ſtring be divided according to thoſe proportions, and this ſtring be ſtopped conſecutively in the different points or diviſions; on being ſtruck, it will expreſs the correſponding ſounds. Thus, if a ſtring ſtretched between two fixed points, as CZ fig. 1. Tab. III. be ſtruck, it will produce a ſound called the prime, firſt or key-note; if it be ſtopped in the middle, one-half of the ſtring will ſound the octave, its length, compared to that of the whole ſtring, being in the proportion of 2 to 1; if two-thirds of the ſtring be cauſed to vibrate, the ſound produced will be the fifth, its length,

length, compared to that of the whole ſtring, being as 2 to 3, and ſo of the reſt.

The higheſt ſound of the octave is expreſſed by the half of the ſtring; and if this half be divided again in the ſame manner or proportion, a higher octave will be obtained, the higheſt note of which will be expreſſed by a quarter of the original ſtring. This quarter may be divided again into a higher octave, and ſo on; therefore, a ſtring ſo divided may expreſs the ſounds of all the keys of an harpſichord or organ.

In regard to thoſe diviſions it muſt be obſerved, that as the notes of the ſecond octave bear the ſame proportion to the firſt note of that octave as the notes of the firſt octave reſpectively bear to the firſt note of that octave, or to the whole ſtring; and as the length of the ſtring expreſſing the firſt note of the ſecond octave, *viz. cZ*, fig. 1. is half the length of CZ, the firſt note of the firſt octave, it follows, that the length of the ſtring of every note in the ſecond octave is half the length of the correſponding note in the firſt octave; thus *g* in the ſecond octave is 120 inches long, and G in the firſt octave is 240 inches long, *viz.* twice 120. Hence, when the diviſions of the firſt octave are aſcertained, in order to find the diviſions of the notes of the ſecond octave, we need only take the half of the lengths expreſſing the notes in the firſt octave. By the very ſame reaſoning it is evident, that to find the diviſions for the third octave, we need only take the halves of the lengths which expreſs the notes of the ſecond octave, or the quarters of thoſe of the firſt octave, and ſo of the reſt.

The ſtring or line CZ, fig. 1. is divided in the above-mentioned manner, and in order to avoid confuſion, the diviſions of the principal notes only of the firſt and ſecond octave are annexed to it. The numbers under the line expreſs the lengths from

from Z to the diviſions to which they ſtand near. The letters juſt over the lines are the names of the notes or ſounds expreſſed by the correſponding lengths of the ſtring. The fractional numbers expreſs the proportion which each particular diviſion bears to the whole ſtring; and the Roman numbers denote the numerical names of each note with reſpect to its diſtance from the firſt, which is always included. For example, ſuppoſe the whole ſtring to be called C, and to be 360 inches long: then, if this ſtring be ſtopped in G, the part GZ will be 240 inches, *viz.* two-thirds of the whole ſtring CZ; the ſound expreſſed by it when ſtruck is called G, and it is the fifth note from C, which is the firſt or key-note. Again, if this ſtring be ſtopped in A, the part AZ will be equal to 216 inches, *viz.* three-fifths of the whole; the ſound produced by it is called A, and is a ſixth to the key-note C, &c.

It is evident, that if any of thoſe diviſions be conſidered as the firſt or key-note, then the other notes, though they retain their alphabetical names, muſt have their numerical names altered accordingly: for example, if we take D for the key-note, then A will be the fifth of it, whereas A was the ſixth when C was conſidered as the key-note; thus alſo B is the third of G, and the ſeventh of C, and ſo on.

Thus much having been premiſed, we may proceed to ſhew the meaning of what is called *the temperament* in a ſyſtem of muſical ſounds, and likewiſe the neceſſity of it. For this purpoſe it is neceſſary to recollect, firſt, that the ſtring, divided in the above-mentioned manner, exhibits the various notes or ſounds of the keys of an harpſichord, the pipes of an organ, &c. Secondly, that thoſe diviſions remain unalterable, ſo that the harpſichord, when tuned, cannot be altered in the courſe of performing on it; and, thirdly, that when any one of

thoſe notes or diviſions is conſidered as the key-note, its ſecond, third, fourth, fifth, &c. muſt bear their reſpective proportions, according to what has been ſaid in the preceding pages.

Now, if amongſt the diviſions of the ſtring CZ, fig. 1. we take D for the firſt or key-note, its length being 320 inches, the length of its fifth muſt be 213⅓ inches, *viz.* two-thirds of 320, that being the proportion which the fifth muſt bear to the key-note; but amongſt the diviſions of the ſtring, there is none equal to 213⅓ inches; therefore, there is not a note among them which may ſerve for a fifth to D: however, as the length of AZ, *viz.* 216, is the neareſt to 213⅓, this A muſt be taken for the fifth of D. It is evident, that this is an imperfect fifth of D; but if, in order to render it perfect, we make AZ equal to 213⅓ inches inſtead of 216, then it will be a redundant ſixth to C, when C is conſidered as the key-note; the beſt expedient, therefore, is to divide the imperfection between the two lengths, *viz.* to make AZ neither ſo long as 216, nor ſo ſhort as 213⅓, which will render the diſagreeable ſenſation, ariſing from the improper length, the leaſt poſſible. This alteration of the juſt lengths of ſtrings, neceſſary for adapting them to ſeveral key-notes, is called *the temperament:* and the beſt temperament in a ſet of muſical ſounds is evidently ſuch a partition of the natural imperfections, as will render all the chords equally and the leaſt diſagreeable poſſible.

What has been exemplified in D and A may be ſaid of all the other notes; ſo that if any one of them be a perfect third, fifth, &c. with reſpect to one key-note, it will be found to be imperfect with reſpect to others. Hence it is manifeſt, firſt, that in a ſet of muſical keys, pipes, or frets, a temperament is abſolutely neceſſary; and, ſecondly, that the harpſichord, organ,

organ, guitar, or any other inſtrument in which the notes are fixed, ſo as not to be alterable by the performer's hands, muſt be imperfect even when tuned in the beſt manner poſſible; for by the temperament we can divide, but not annihilate, the imperfection.

Other inſtruments, in which the notes are not fixed, as the violin, violoncello, &c. are perfect, becauſe the performer ſtops the ſtrings upon them in different places, even for ſounding the notes of the ſame name. Thus a ſkilful performer, in order to ſound A, will ſtop the ſtring a little farther from the bridge when he plays in the key of C, *viz.* when C is conſidered as the key-note, than when he plays in the key of D.

Moſt people imagine, that the ſcale of muſic is capable of many different temperaments; and, agreeable to this ſuppoſition, the writers on harmonics have propoſed different temperaments; but it will be ſhewn in the ſequel, that the nature of the ſcale admits of only one temperament capable of rendering the imperfection and the harmony equal throughout; and that it is impoſſible to form a different and more advantageous ſcale.

Before we begin with the inveſtigation of this ſubject, it will be neceſſary to explain certain principles, the want of which may poſſibly raiſe ſome doubts in the minds of thoſe perſons who are not much acquainted with the theory of muſical ſounds. In the firſt place it muſt be obſerved, that the proportion of 2 to 3 for the fifth, the proportion of 1 to 2 for the octave, and in ſhort the proportions of all the notes, are not aſſumed at pleaſure; but they have been determined from conſtant experience, *viz.* from the agreeable or diſagreeable effects produced when two different notes are ſounded at the ſame time.

To render this more evident, let two ſtrings equal in every reſpect be ſtruck at the ſame time, and they will expreſs the ſame ſound preciſely, ſo that no ear can perceive any difference between them, and it is almoſt impoſſible to diſtinguiſh whether the ſound ariſes from two ſtrings, or from one only, excepting from the loudneſs. But if one of thoſe ſtrings be ſucceſſively ſtopped in different parts of its length, whilſt the other remains open as before, and if at every time they be both ſtruck together, their combined ſounds will be found to produce different effects, *viz.* ſometimes more or leſs pleaſing, and at other times more or leſs diſagreeable. When the combinations of the two ſounds are agreeable, they are called *concords*; and when diſagreeable, they are called *diſcords*.

Experience evinces, that the beſt concord is when the length of one ſtring is to the length of the other as 1 to 2, every other circumſtance being the ſame in both. This proportion forms the octave. The next beſt concord is the fifth, *viz.* when the lengths of the two ſtrings are as 2 to 3, after which come the proportions of 3 to 4, 4 to 5, 3 to 5, 5 to 6, and 5 to 8, for the other concords. The other proportions beſides theſe are diſagreeable in a greater or leſs degree, unleſs they are greater than the proportion of 1 to 2; but in that caſe it will be found, that the proportions which produce agreeable combinations are the double, quadruple, octuple, &c. of thoſe mentioned above, *viz.* are their octaves, double octaves, &c.: thus the proportion of 1 to 4 produces a very agreeable concord, becauſe 1 to 4 is the double of 1 to 2, *viz.* it expreſſes a double octave.

Secondly, it appears, from the foregoing obſervations, that if we have the length of a ſtring, or the proportion of a note in any part of the ſtring, we may eaſily find its octaves by taking

taking its double, or its half, or the double of the double, &c.: for inſtance, in fig. 1. if *cZ* be given equal to 90 inches, we may find its octave below by taking twice 90, *viz.* 180, or the octave of this octave, which is 360, *viz.* equal to twice 180, or to four times 90; and, on the other ſide, we may find the octave above of the given note by taking its half, which is 45, &c.

It is now neceſſary to ſhew why within the octave there are admitted only thirteen different notes, *viz.* eight principal ones, and five others, called ſharps and flats.

The line XY, fig. 2. repreſents a muſical ſtring, the length of which is ſuppoſed to be divided into 13286025 equal parts *. On one ſide of this line there are the diviſions of ſeven ſucceſſive octaves, *viz.* the half of XZ, a quarter of it, &c.; and on the other ſide are the diviſions of a ſeries of fifths, *viz.* the fifth of the whole ſtring, the fifth of this fifth, and ſo on, which are found by taking two-thirds of the whole ſtring, then two-thirds of thoſe two thirds, and ſo on.

Here we take notice only of the octaves and fifths, becauſe they are the principal and the beſt concords; ſo that a temperament being required, it is neceſſary firſt to take care, that theſe concords be not rendered inſufferable to the ear, the reſt admitting of a greater latitude in the temperament or deviation from the perfect ſtate. Beſides, it will appear in the ſequel, that all the other notes are derived from the ſeries of ſucceſſive fifths.

In whatever key a piece of muſic is performed, its fifth is the moſt predominant of its concords; and as the notes of

* This number has been choſen, becauſe the line may in that caſe be divided into the neceſſary number of ſucceſſive octaves and fifths, without any common fractions, which renders the operation more eaſy and more perſpicuous; otherwiſe there might have been aſſumed any other number.

muſic muſt be ſo ordered as that, for the ſake of modulation, any note may be conſidered as the key-note; therefore having found the fifth of the whole ſtring by taking two-thirds of its length, which gives a note called G, we muſt ſuppoſe, that this G may be conſidered as the key-note, conſequently muſt find its fifth, which gives D, as ſhewn in the figure, and ſo on, until we find one of thoſe ſucceſſive fifths, which coincides with one of the ſucceſſive octaves; for after that, to find more ſucceſſive fifths would be only repeating the ſame thing over again.

Indeed, if we carry the ſucceſſion of octaves and of fifths indefinitely far, we ſhall find, that no one of the fifths ever coincides perfectly with one of the octaves, and therefore the diviſion would have no end. However, as the length of the ſeventh octave comes ſo very near to the twelfth fifth, we muſt be contented with taking this ſeventh octave for the fifth of F, the difference between this note and the perfect fifth of F being about the hundredth part of its length; whereas, if we carry on the ſucceſſion of fifths and of octaves, we ſhall find, that amongſt thirty and more fifths none comes nearer to one of the octaves than the above-mentioned one, as may be ſeen in the following table, which contains a ſeries of ſucceſſive octaves, and another ſeries of ſucceſſive fifths, as in fig. 2.; but in the table the diviſions are carried farther on than in the figure:

Octaves.	Fifths.
1328025	1328025.
6643012,5	8857350.
33215 06,25	5904900.
1660753,125	3936600.
830376,5625	2624400.

Octaves

Octaves.	Fifths.
415188,28125	1749600.
207594,140625	1166400.
☞ 103797,0703125	777600.
51898,5351 +	518400.
25949,2675 +	345600.
12974,6387 +	230400.
6487,3193 +	153600.
3243,654 +	☞ 102400.
1621,827 +	68266,6 +
810,913 +	45511 +
405,456 +	30340,7 +
202,728 +	20227 +
101,364 +	6742,3 +
50,662 +	4494,8 +
25,331 +	2996,6 +
12,665 +	1997,7 +
	1331,8 +
	887,8 +
	592 +
	394,6 +
	263 +
	175,4 +
	117 +
	78 +
	52 +
	34,6 +
	23 +
	15,3 +

The number of fifths then in this ſeries is twelve, and as, from what has been ſaid above, when the diviſion expreſſing a certain note has been aſſigned in any part of a ſtring, we may

eaſily find all its octaves above and below, it follows, that by finding all the octaves of thoſe twelve diviſions we ſhall have twelve diſtinct notes within half the ſtring, *viz.* within the firſt octave of the whole ſtring; to which, if the ſound of the whole ſtring be added, we ſhall have thirteen different ſounds, which ſhews why an octave comprehends neither more nor leſs than thirteen notes.

Without dwelling any longer upon the names or number of thoſe notes, I ſhall immediately proceed to find out the temperament.

It has been ſhewn above, and it is expreſſed in fig. 2. that the length of the ſtring for the laſt fifth is ſhorter than the length of the laſt octave, and alſo that one of them muſt be neceſſarily taken for both purpoſes; but here we muſt conſult nature, examining by the ear which of the two is leaſt diſagreeable. This, however, is ſoon decided; for imperfect octaves are quite inſufferable, whereas a certain degree of imperfection in the fifths is tolerable; therefore we are neceſſitated to leave the octaves perfect, and to let the ſeventh octave ſerve for the fifth of F. In this caſe it is evident, that each of the notes in the ſucceſſion of fifths is a perfect fifth to its preceding note, excepting the laſt, which would be by much too flat, and therefore it is neceſſary to divide the imperfection equally among them all.

For this purpoſe it muſt be conſidered, that as the twelve ſucceſſive fifths, together with the whole ſtring or firſt note, are each two-thirds of its preceding note; they form a geometrical ſeries, the ratio of which is $\frac{3}{2}$, its extremes are 13286025 and 102400, and the number of terms is 13. But becauſe inſtead of 102400, which is the laſt fifth, we muſt take the number 103797,0703125 (*viz.* the length of the laſt octave) for

for the laſt term of the ſeries; therefore the problem is reduced to the finding out of eleven mean proportionals between the two numbers 1328025 and 103797,0703125.

It is demonſtrated in almoſt every treatiſe on algebra and arithmetic, that in a geometrical progreſſion, as the above-mentioned one, the firſt or ſmalleſt extreme is to the laſt or greateſt extreme as unity is to a power of the ratio, the index of which is equal to the number of terms leſs one. Hence, in our caſe, in which the number of terms, including the two extremes, is 13, we ſhall have 103797,0703125 : 1328025 :: 1 : R^{12}, from which the ratio is found by dividing the ſecond number by the firſt, and extracting the twelfth root from the quotient, *viz.* $\frac{1328025}{103797,0703125} = 128$; and $\sqrt[12]{128} = 1,4983069$, which is the ratio ſought.

The ratio having been aſcertained, the ſucceſſion of tempered fifths is thus eaſily determined; *viz.* divide the length of the whole ſtring by this ratio, and the quotient gives the firſt tempered fifth; divide this fifth by the ſame ratio, and the quotient gives the ſecond tempered fifth; divide this ſecond fifth by the ſame ratio, and ſo on till the laſt fifth, which comes out equal to 103797, 21735 (ſee fig. 3.) which is ſo nearly equal to the length of the ſeventh octave, that the difference is truly inſignificant; but, if greater accuracy were required, we need only extract the proper root of 128 to a greater number of decimals.

Fig. 3. ſhews the diviſions of the ſtring XZ tempered in the above mentioned manner; *viz.* the ſucceſſive fifths have been aſcertained firſt, and then, by taking their octaves, the whole ſet of diviſions has been completed. By comparing this figure with fig. 2. one may eaſily perceive, how ſmall is

the difference between the perfect fifths of the latter, and the tempered ones of the former.

The diviſions, thus aſcertained, form a ſeries of notes, in which the octaves only are perfect; but all the fifths, all the thirds, and in ſhort all the chords of the ſame denomination, are equally tempered throughout: ſo that whichever of them is taken for the key-note, its fifth, ſixth, &c. will have always the ſame proportion to it, and conſequently will always produce the ſame harmony when ſounded with it.

It is evident, that, beſides this, there can be no other temperament capable of producing equal harmony; for when the extremes of a geometrical ſeries and number of mean proportionals are given, there can be but one ſet of thoſe means: thus, if we are to find two mean proportionals between the numbers 2 and 16, theſe are neceſſarily 4 and 8; nor is it poſſible to aſſign any others.

If, on the other hand, we endeavour to find a better temperament by introducing more than thirteen notes within the limits of an octave, we ſhall find it impracticable, becauſe it has been ſhewn, in the preceding pages, that after the number thirteen, if the ſucceſſion of fifths be carried farther on, they will recede more from a coincidence with any one of the octaves.

This explanation of the nature, origin, and neceſſity of the temperament has been thought neceſſary for the ſake of perſpicuity; but the ſame end may be obtained by the following eaſier method. As the thirteen notes of an octave muſt be arranged ſo, that whichever of them be taken for the firſt or key-note, the ſecond, third, fourth, &c. may bear the ſame conſtant proportion to it; therefore it follows, that they muſt be in a geometrical proportion one of the other, ſo as to form a

ſeries

ſeries of thirteen numbers, the extremes of which are the whole ſtring and its half, *viz.* any number and its half. The ratio of this ſeries is found in the ſame manner as in the other ſeries, *viz.* the greateſt extreme is divided by the leaſt, and the twelfth root of the quotient is the ratio ſought. But the extremes are any aſſumed number and its half: and as the quotient of a number divided by the half of the ſame number is always equal to two; therefore, whatever be the length of the ſtring, the ratio is always $\sqrt[12]{2} = 1{,}0594+$, and if the length of the whole ſtring be divided by this ratio, *viz.* 1,0594+, the quotient will be the length of the ſtring expreſſing the ſecond note, which, divided by the ſame ratio, gives the third note, and ſo on; or elſe, inſtead of dividing the length of the whole ſtring by the ratio, you may multiply the half of it by the ratio, the product of which will give the ſeventh note, which multiplied by the ſame ratio gives the ſixth, and ſo on in a retrograde order, which will give the tempered notes of the octaves as well as the former method. By this means the following diviſions for the notes of an octave have been calculated, the length of the whole ſtring having been ſuppoſed equal to 100000.

I.	100000.
* b	94387.
II.	89090.
* b	84090.
III.	79370.
IV.	74915.
* b	70710.
V.	66743.
* b	62997.
VI.	59462.
* b	56123.

VII. 52973.

VIII. 50000.

If a monochord be divided in this manner, and a harpſichord tuned by it, this inſtrument will then be tuned ſo, that whichever note be taken for the firſt or key-note, its fifth, ſixth, &c. will produce the ſame effect reſpectively.

Thus far I have endeavoured to explain this ſubject in the moſt familiar manner, avoiding as much as poſſible the mathematical language and ſymbols; having found, by experience, that intricate mathematical diſquiſitions, eſpecially on this ſubject, are underſtood only by a few able mathematicians, but that they are neither comprehended, nor even read, by thoſe who might wiſh to underſtand, or to uſe them. It is now neceſſary to conſider this ſubject with reſpect to the practice.

At preſent, the harpſichords and organs are commonly tuned ſo, that ſome concords are very agreeable to the ear, whilſt others are quite intolerable; or, in other words, when the performer plays in certain keys, the harmony is very pleaſing, in others the harmony is juſt tolerable, and in ſome other keys the harmony is quite diſagreeable.

The beſt keys to be played in are the keys of C, of F, of E flat, of B flat, of G and of D in the major mood, and the keys of C, of D, of A, and of B, in the minor mood. Next to thoſe come the leſs agreeable keys of A, A flat, and E in the major mood; beſides thoſe, the reſt are diſagreeable in a greater or leſs degree, ſo that out of twelve keys *, which, on account of the two moods, *viz.* the major and the minor, become twenty-four, there are hardly fourteen that can be

* The octave muſt be excepted, in this reſpect it being the ſame as the firſt note.

uſed; and for this reaſon moſt of the modern compoſitions in muſic are written in thoſe keys.

So far the common method of tuning anſwers ſome purpoſe; for as long as the performer is to play in certain keys only, it is much better to have them tuned in the moſt advantageous manner, than to let thoſe be tuned in a leſs perfect manner for the ſake of others, which he does not intend to uſe. Hence the great harpſichord players generally have their inſtruments tuned in a peculiar manner, *viz.* ſo as to give the moſt advantageous effect to thoſe concords which they more frequently uſe in their compoſitions. And hence alſo, the harpſichords and organs are always tuned different from each other, unleſs they be tuned by the ſame perſon with equal attention, and without any particular inſtructions.

This practice cannot conveniently be laid aſide, *viz.* when the inſtrument is to be tuned for ſolo playing; and for a certain ſtyle of muſic, it is very proper to tune it ſo as to give the greateſt effect to thoſe combinations of ſounds, which are moſtly uſed in thoſe compoſitions. But the caſe is far different when the inſtrument is to ſerve for accompanying other inſtruments in every ſort of muſic, or the voices of good ſingers; for then the diſagreement becomes very audible; and for this purpoſe the harpſichord or organ ought to be tuned according to the above demonſtrated temperament of equal harmony, which is the only one that can poſſibly take place.

When the compoſitions of old maſters are performed in concert, and with the organ or harpſichord tuned in the common manner, the effect is frequently very diſagreeable. This is particularly the caſe with the ſongs of HANDEL, GALLUPPI, LEO, PERGOLESE, and others, who wrote in a great variety

of keys, and very often in thoſe, for which the common way of tuning is not at all calculated.

In order to hear the effect of the above-mentioned temperament of equal harmony, I had a monochord made in a very accurate manner, and upon it I laid down the diviſions for the thirteen notes of an octave properly tempered in the manner explained above. After a great deal of trouble in adjuſting the moveable fret, correcting the diviſions, &c. I at laſt ſucceeded ſo well as to render the diviſions exact within at leaſt the 300th part of an inch, and every part of the inſtrument was rendered ſufficiently ſteady and unalterable.

This being done, I had a large harpſichord, with a ſingle uniſon (in order to judge the better of the effect), tuned very accurately by the help of the monochord. With this inſtrument, in whatever key the performer played, the harmony was perfectly equal throughout, and the effect was the ſame as if one played in the key of E natural on a harpſichord tuned in the uſual manner.

I ſhall, therefore, conclude with ſaying, that when the harpſichord, organ, &c. is to ſerve for ſolo playing, and for a particular ſort of muſic, it is proper to tune in the uſual manner, *viz.* ſo as to give the greateſt effect to thoſe concords which occur more frequently in that ſort of muſic; but that when the inſtrument is to ſerve for accompanying other inſtruments or human voices, and eſpecially when modulations and tranſpoſitions are to be practiſed, then it muſt be tuned according to the temperament of equal harmony, which has been explained in the preceding pages.

XVI. *Deſcription of a new Electrical Inſtrument capable of collecting together a diffuſed or little condenſed Quantity of Electricity.* *By Mr.* Tiberius Cavallo, *F. R. S.*

Read April 10, 1788.

ONE of the principal deſiderata in practical electricity has been a method of aſcertaining the preſence and quality of ſuch diffuſed or weak electricity as could not immediately affect an electrometer; of this nature is the electricity produced by effervеſcences and other proceſſes, the electricity of the atmoſphere in ſerene and warm weather, &c.

M. Volta's condenſer, which is deſcribed in Volume LXXII. of the Philoſophical Tranſactions, was the firſt attempt of the kind, and, indeed, when this inſtrument is in good order, it anſwers exceedingly well; but the difficulty of conſtructing and of preſerving it, added to the frequent uncertainty of the reſult (it being difficult to ſay, whether in certain caſes the electricity obtained comes from the inſtrument itſelf, or from the ſubſtance in queſtion) have occaſioned its being little, if at all, uſed by thoſe who ſtudy the ſubject of electricity.

Mr. Bennet's doubler, which is deſcribed in Volume LXXVII. of the Philoſophical Tranſactions, was alſo intended to manifeſt ſmall, and otherwiſe unperceivable, quantities of electricity; but from the experiments and obſervations, which I had the honour of laying before the Royal Society in November laſt, it ſeems to be clearly ſhewn, that this doubler

bler cannot be of any uſe, on account of its being naturally always electrified.

In the ſame Paper of laſt November, I likewiſe ſhortly mentioned a method which I had uſed for collecting diffuſed quantities of electricity. Since that time, I have improved the method; and, after ſeveral alterations, have conſtructed an inſtrument for the purpoſe, which, to all my friends who are converſant in electricity, as well as to myſelf, ſeems to be free from all thoſe faults which render M. VOLTA's and Mr. BENNET's inſtruments of little, if at all of any uſe. It ſeems, therefore, that the following deſcription of this new inſtrument may be of uſe to thoſe who are purſuing electrical experiments and inveſtigations.

The properties of this machine, which from its office may be called *a collector of electricity*, are, firſt, that when connected with the atmoſphere, the rain, or in ſhort with any body which produces electricity ſlowly, or which contains that power in a very rarefied manner, it collects the electricity, and afterwards renders both the preſence and quality of it manifeſt, by communicating it to an electrometer. Secondly, This collecting power, by increaſing the ſize of the inſtrument, and eſpecially by uſing a ſecond or ſmaller inſtrument of the like ſort to collect the electricity from the former, may be augmented to any degree. Thirdly, It is conſtructed, managed, and preſerved with eaſe and certainty; and it never gives, nor can it give, an equivocal reſult, as I have proved experimentally, and as will appear by conſidering its conſtruction.

The annexed drawing exhibits two perſpective views of this collector. Fig. 1. (Tab. IV.) ſhews the inſtrument in the ſtate of collecting the electricity; and fig. 2. ſhews it in the ſtate in which the collected electricity is to be rendered manifeſt. An

electrometer

electrometer is annexed to each. The letters of reference indicate the ſame parts in both figures.

ABCD is a flat tin plate, thirteen inches long and eight inches broad; to the two ſhorter ſides of which are ſoldered two tin tubes AD and BC, which are open at both ends. DE and CF are two glaſs ſticks covered with ſealing wax by means of heat, and not by diſſolving the ſealing wax in ſpirits. They are cemented into the lower apertures of the tin tubes, and alſo in the wooden bottom of the frame or machine at E and F, ſo that the tin plate ABCD is ſupported by thoſe glaſs ſticks in a vertical poſition, and is exceedingly well inſulated. GHILKM and NOPV are two frames of wood, which being faſtened to the bottom boards, by means of braſs hinges, may be placed ſo as to ſtand in an upright poſition and parallel to the tin plate, as ſhewn in fig. 1. or they may be opened, and laid upon the table which ſupports the inſtrument, as ſhewn in fig. 2. The inward ſurfaces of thoſe frames from their middle upwards is covered with gilt paper XY; but it would be better to cover them with tin plates, hammered very flat. When the lateral frames ſtand ſtraight up, they do not touch the tin plate; but they ſtand at about one-fifth part of an inch aſunder. They are alſo a little ſhorter than the tin plate, in order that they might not touch the tin tubes AD, BC. In the middle of the upper part of each lateral frame is a ſmall flat piece of wood S and T, with a braſs hook; the uſe of which is to hold up the frames without the danger of their falling down when not required, and at the ſame time it prevents their coming nearer to the tin plate than the proper limit. It is evident, that when the inſtrument ſtands as ſhewn in fig. 1. the gilt ſurface of the paper XY, which covers the inſide of the lateral frames, ſtands contiguous and parallel to the tin plate.

When

When the inſtrument is to be uſed, it muſt be placed upon a table, a window, or other convenient ſupport, a bottle electrometer is placed near it, and is connected, by means of a wire, with one of the tin tubes AD, BC; and by another conducting communication the tin plate muſt be connected with the electrified ſubſtance, the electricity of which is required to be collected on the plate ABCD: thus, for inſtance, if it be required to collect the electricity of the rain, or of the air, the inſtrument being placed near a window, a long wire muſt be put with one extremity into the aperture A or B of one of the tin tubes, and with the other extremity projecting out of the window. If it be required to collect the electricity produced by evaporation, a ſmall tin pan, having a wire or foot of about ſix inches in length, muſt be put upon one of the tin tubes, ſo that the wire going into the tube the pan may ſtand about two or three inches above the inſtrument. A lighted coal is then put into the pan, and a few drops of water poured upon it will produce the deſired effect. Thus far may ſuffice with reſpect to the mechanical deſcription of the inſtrument: the power and uſe of it will be made apparent by the following experiments.

EXP. 1. Communicate to the tin plate ABCD a quantity of electricity, for inſtance, as much as would very ſenſibly affect a common cork-ball electrometer; then, if the lateral frames GHM, NOP, ſtand upright, as in fig. 1. the electrometer W will ſhew no divergency; but if the frames are opened and let down, as in fig. 2. the balls of the electrometer W will immediately repel each other, and by the approach of an excited piece of ſealing-wax, the quality of the electricity may be eaſily aſcertained after the uſual manner.—Put up the lateral frames again, and the electricity will apparently vaniſh;—let them down, and the electricity will re-appear, and ſo on.

If

If you touch any part of the tin plate or tin tubes with your finger, the electricity is thereby intirely removed, and that will be the caſe whether the lateral frames are up or down.

Exp. II. Take an extended piece of tinfoil, about four yards ſquare, and, holding it by a ſilk thread, electrify it ſo weakly as not to be capable of affecting an electrometer; then bring it in contact with the tin plate of the collector, whilſt the lateral frames are up. This done, remove the tin-foil, let down the lateral frames one after the other, and on doing this, the electrometer W will immediately manifeſt a conſiderable degree of electricity. But if the electrometer were to ſhew no ſenſible degree of electricity, a ſmaller collector, *viz.* one having a tin plate of about four ſquare inches, muſt be brought into contact with the tin plate of the large collector, whilſt the lateral frames of the latter only are down; and then the ſmall collector being removed from the large one, its lateral frames are opened, and its tin plate is preſented to an electrometer, which will thereby be electrified to a much greater degree than the electrometer W was by the large collector.

Exp. III. Let a common cork-ball electrometer be faſtened to an inſulated conductor, having about two or three ſquare feet of ſurface, and communicate to it ſuch a quantity of electricity as may be ſufficient to let the balls of the electrometer ſtand at about one inch aſunder. In this ſtate bring the conductor in contact with the tin plate of the collector for a very ſhort time, and it will be found, that the balls of its electrometer will immediately approach and touch each other, ſhewing that the electricity of the conductor is gone to the plate of the collector; and, in fact, if you let down the lateral frames, the balls of the electrometer W will immediately repel each other to a very great degree.

It ſeems, therefore, to be clearly ſhewn by theſe experiments, that the tin plate of this inſtrument can collect and retain a vaſt quantity of electricity, when the conducting ſurfaces of the lateral frames are contiguous to it, in compariſon to that quantity which it can either collect or retain when thoſe ſurfaces are removed from its vicinity.

The quantity of electricity, which the tin plate ABCD is capable of collecting, principally depends on three circumſtances, *viz.* 1ſt, on the diſtance between the tin plate and the conducting lateral ſurfaces; the ſmaller that diſtance is, the greater being the collecting power; 2dly, on the ſize of the inſtrument; and, 3dly, on the quantity of electricity poſſeſſed by the body from which it muſt be collected or taken away.

I need not expatiate on the principle upon which the action of this inſtrument depends; this being the ſame as that of the electrophorus, of M. VOLTA's condenſer, and of many other electrical experiments; namely, that a body has a much greater capacity for holding electricity when its ſurface is contiguous to a conductor which can eaſily acquire the contrary electricity, than when it ſtands not in that ſituation.

I ſhall laſtly add, that having actually uſed this new inſtrument in ſeveral experiments, I have found it to anſwer perfectly well; one of its principal recommendations being the certainty of its operation.

XVII. *On the Converſion of a Mixture of dephlogiſticated and phlogiſticated Air into nitrous Acid, by the electric Spark.* By Henry Cavendiſh, *Eſq. F. R. S. and A. S.*

Read April 17, 1788.

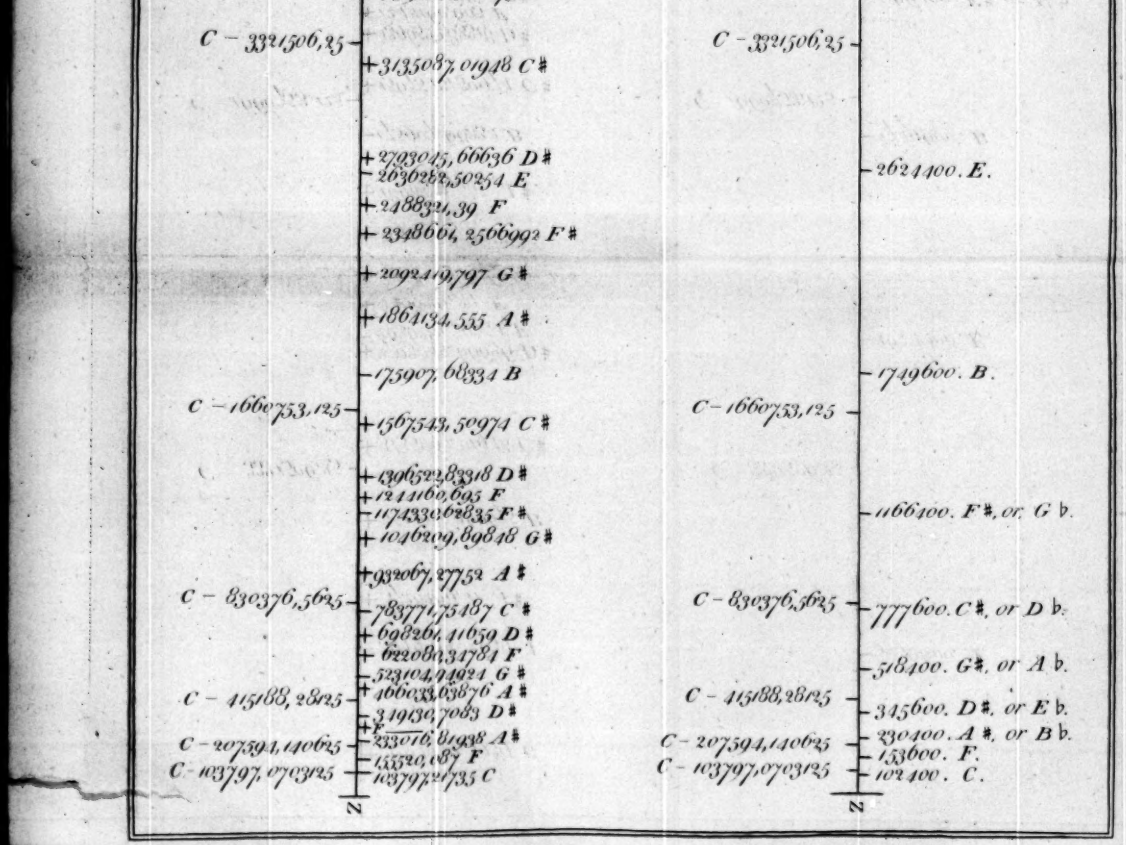
IN Volume LXXV. of the Philoſophical Tranſactions, p. 372. I related an experiment, which ſhewed, that by paſſing repeated electric ſparks through a mixture of atmoſpheric and dephlogiſticated air, confined in a bent glaſs tube by columns of ſoap-lees and quickſilver, the air was converted into nitrous acid, which united to the ſoap-lees and formed nitre. But as this experiment has ſince been tried by ſome perſons of diſtinguiſhed ability in ſuch purſuits without ſucceſs, I thought it right to take ſome meaſures to authenticate the truth of it. For this purpoſe, I requeſted Mr. GILPIN, Clerk of the Royal Society, to repeat the experiment, and deſired ſome of the Gentlemen moſt converſant with theſe ſubjects to be preſent at putting the materials together, and at the examination of the produce.

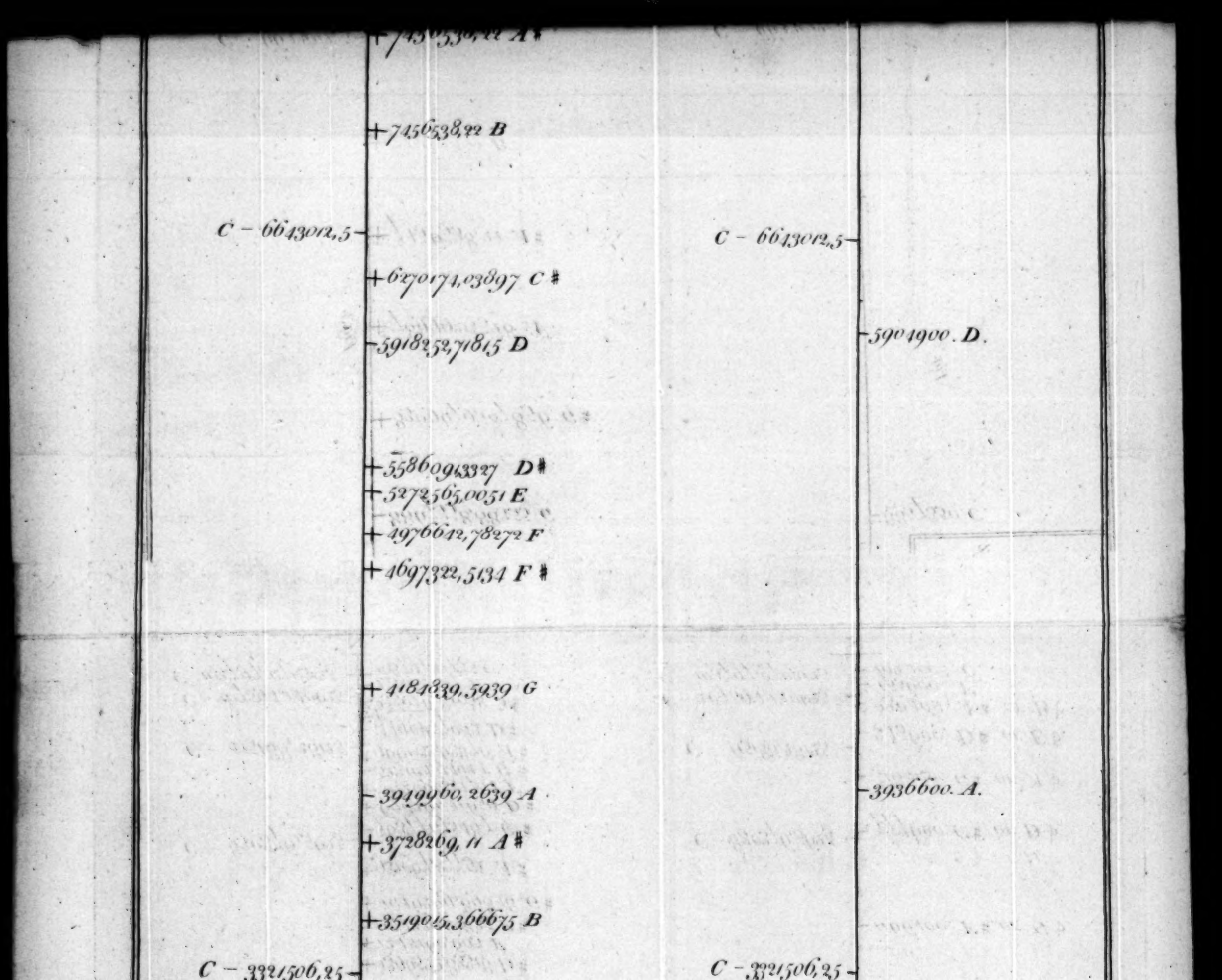
This laborious experiment Mr. GILPIN was ſo good as to undertake. It was performed in the ſame manner, and with the ſame apparatus, which was uſed in my own experiments, and which is deſcribed in the beginning of the above-mentioned Paper, and is accompanied with a drawing. The

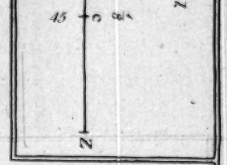
method uſed for introducing air into the bent tube, was that deſcribed in the laſt paragraph of p. 373. in that Paper, by means of the apparatus repreſented in fig. 3. or the reſervoir, as I ſhall call it. The ſoap-lees, like thoſe of my own experiments, were prepared from ſalt of tartar, and were of ſuch ſtrength as to yield $\frac{1}{10}$ of their weight of nitre when ſaturated with nitrous acid. The dephlogiſticated air was prepared from turbith mineral, and ſeemed by the nitrous teſt to contain about $\frac{1}{10}$ part of phlogiſticated air.

On December 6, 1787, in the preſence of Sir JOSEPH BANKS, Dr. BLAGDEN, Dr. DOLLFUSS, Dr. FORDYCE, Dr. J. HUNTER, and Mr. MACIE, the materials were put together. The quantity of ſoap-lees, introduced into the bent tube, was 180 meaſures, each of which contained one grain of quickſilver; and, as the bore of the tube was rather more than one-third of an inch in diameter, it formed a column of five or ſix-tenths of an inch in length, which, by the introduction of the air, was divided into two parts, one reſting on the quickſilver in one leg of the tube, and the other on that in the other leg. The dephlogiſticated air was mixed with one-third part of its bulk of atmoſpheric air of the room in a ſeparate jar, and the reſervoir was filled with the mixture; and from thence Mr. GILPIN, as occaſion required, forced air into the bent tube, to ſupply the place of that abſorbed by means of the electric ſpark.

From what has been ſaid, it appears, that the mixture employed contained a leſs proportion of common air than that uſed in either of my experiments. This made it neceſſary for Mr. GILPIN now and then to introduce ſome common air by


C − 3321506,25
+ 3135087,01948 C#
+ 2793045,66636 D#
− 2630282,50254 E
+ 2488324,39 F
+ 2348664,2566992 F#
+ 2092449,797 G#
+ 1864134,555 A#
− 1759076,88334 B
C − 1660753,125
+ 1567543,50974 C#
+ 1396522,83318 D#
+ 1241160,695 F
− 1174330,62835 F#
+ 1016209,89848 G#
+ 932067,27752 A#
C − 830376,5625
− 783774,75187 C#
+ 698261,41659 D#
+ 622081,31781 F
− 523104,44921 G#
+ 466033,03876 A#
C − 415188,28125
+ 349430,7083 D#
+ 311516,89938 A#
C − 207594,140625
− 175520,087 F
C − 103797,070325
− 103792,2735 C
N
C − 3321506,25
2624400. E.
1749600. B.
C − 1660753,125
1166400. F#, or G b.
C − 830376,5625
777600. C#, or D b.
518400. G#, or A b.
C − 415188,28125
345600. D#, or E b.
C − 207594,140625
230400. A#, or B b.
153600. F.
C − 103797,070325
102400. C.
N


+745538,42 B
C – 6643012,5
C – 6643012,5
+6270174,03897 C #
5918252,71815 D
5901900. D.
+5586094,3327 D #
+5272565,0051 E
+4976642,78272 F
+4697322,5134 F #
+4184239,5939 G
3919960,2639 A
3936600. A.
+3728269,11 A #
+3519045,366675 B
C – 3321506,25
C – 3321506,25



8867358,88355 G

8369679,187856 G#

Fig. 2.

7899920,528 A

7456538,22 A#

7456538,22 B

C – 6643012,5

6270171,03897 C#

5918253,71845 D

5586091,3327 D#

8857350. C.

Fig. 2.

C – 6643012,5

5901900. D.

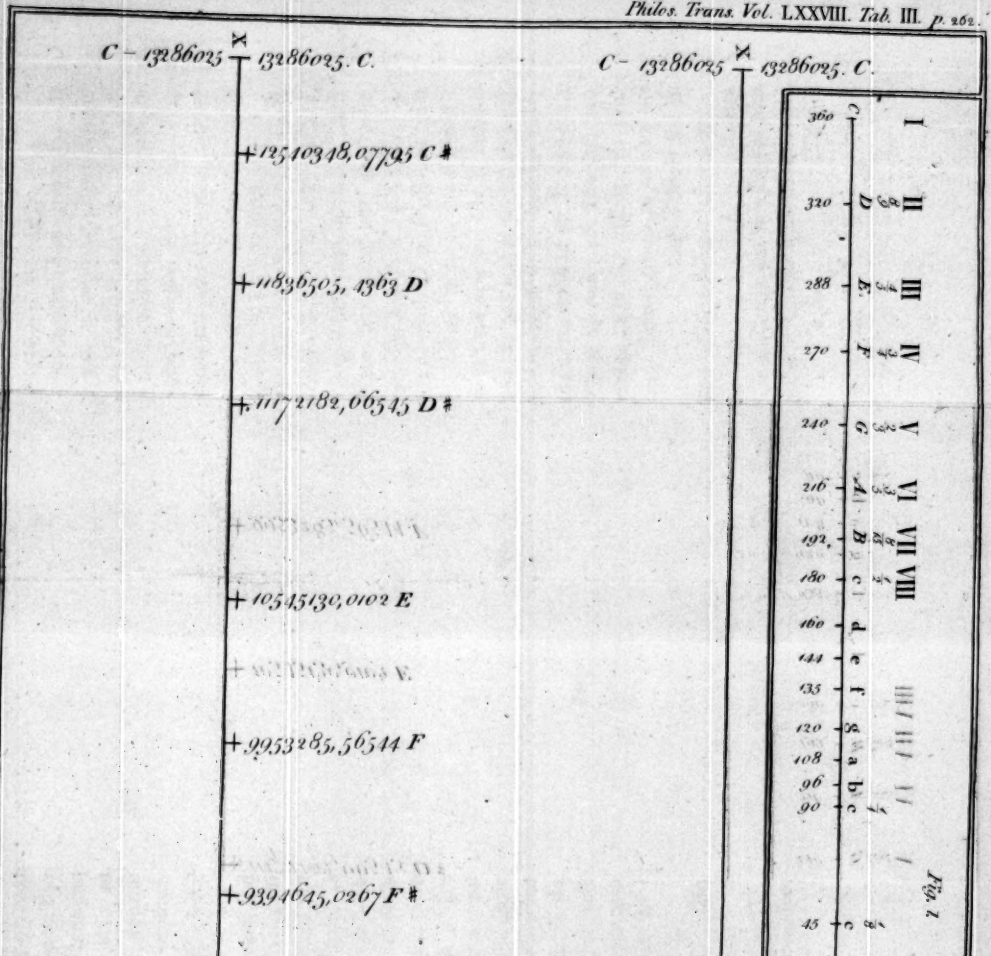

C − 13286025
13286025. C.
+125 10348, 07795 C♯
+11836505, 4363 D
+11721182, 66545 D♯
+10545130, 0102 E
+9953285, 56544 F
+9391645, 0267 F♯
C − 13286025
13286025. C.
360
320
288
270
240
216
192
180
160
144
135
120
108
96
90
45
C
D
E
F
G
A
B
c
d
e
f
g
a
b
c
I
II
III
IV
V
VI
VII
VIII

Fig. 1

means of the bent tube repreſented in fig. 3. of the above-mentioned Paper, whenever from the ſlowneſs of the abſorption he thought there was too ſmall a proportion of phlogiſticated air in the tube.

My reaſon for this manner of proceeding was, that as my firſt experiment ſeemed to ſhew, that the dephlogiſticated air ought to be in a rather greater proportion to the phlogiſticated than the latter did, I was ſomewhat uncertain as to the proper quantities, and doubted whether I could proportion them in ſuch manner as that it ſhould not be neceſſary, during the courſe of the experiment, to add either dephlogiſticated or common air. I therefore mixed the airs in ſuch proportion, that I was ſure there could be no occaſion to add the former; ſince it was much eaſier, as well as more unexceptionable, to add common air than dephlogiſticated air.

On December 24, as the air in the reſervoir was almoſt all uſed, this apparatus was again filled in the preſence of moſt of the above-mentioned Gentlemen, with a mixture of the ſame dephlogiſticated air and common air, in the ſame proportions as before; and the ſame thing was repeated on January 19.

On January 23, the bent tube was, by accident, raiſed out of one of the glaſſes of mercury into which it was inverted, by which it was filled with air, and a good deal of the ſoap-lees were loſt; there, however, was enough remaining for examination.

On January 28, and 29, the produce of this experiment was examined in the preſence of Sir Joseph Banks, Dr. Blagden, Dr. Dollfuss, Dr. Fordyce, Dr. Heberden, Dr. J. Hunter, Mr. Macie, and Dr. Watson. It appeared that 9290 meaſures of the mixed air had been forced into the bent

bent tube from the reſervoir*. Beſides this, Mr. GILPIN had at different times introduced 872 meaſures of common air, which makes in all 10162 of air, conſiſting of 6968 of dephlogiſticated air, and 3194 of common air. But as there were 900 meaſures of air remaining in the tube when the accident happened, the quantity abſorbed was only 9262; but this is a much greater quantity that what from my own experiments ſeemed neceſſary for this quantity of ſoap-lees.

The ſoap-lees were poured into a ſmall glaſs cup, and the tube waſhed with a little diſtilled water, in order that as little as poſſible might be loſt. As they were by this means conſiderably diluted, they were evaporated to dryneſs; but it was difficult to eſtimate the quantity of the ſaline reſiduum, as it was mixed with a few particles of mercury.

Some vitriolic acid, dropped on a little of this reſiduum, yielded a ſmell of nitrous acid, the ſame as when dropped on nitre phlogiſticated by expoſure to the fire in a covered crucible; but it was thought leſs ſtrong. The remainder was diſſolved in a ſmall quantity of diſtilled water, and the following experiments were tried with the ſolution.

It did not at all diſcolour paper tinged with the juice of blue flowers.

It left a nauſeous taſte in the mouth like ſolutions of mercury, and moſt other metallic ſubſtances.

Paper dipped into it, and dried, burnt with ſome appearance of deflagration, but not ſo ſtrongly or uniformly as when dipped in a ſolution of nitre. The marks of deflagration, however, were ſtronger than when the Paper was dipped into a ſolution

* The method of aſcertaining the quantity of air forced in was by weighing the reſervoir, as mentioned in the above-mentioned Paper, p. 374.

of

of mercury in ſpirit of nitre, but not ſo ſtrong as when equal parts of this ſolution and ſolution of nitre were uſed.

A ſolution of fixed vegetable alkali, dropped into ſome of it diluted, produced a ſlight reddiſh-brown precipitate, which afterwards aſſumed a greeniſh colour.

A bit of bright copper being dipped into it, acquired an evident whitiſh colour, though not ſo white as when dipped into the ſolution of mercury in ſpirit of nitre.

From theſe experiments it appears, that the mixture of the two airs was actually converted into nitrous acid, only the experiment was continued too long, ſo that the quantity of air abſorbed was greater than in my experiments, and the acid produced was ſufficient, not only to ſaturate the ſoap-lees, but alſo to diſſolve ſome of the mercury. The truth of the latter part is proved by the metallic taſte of the reſiduum, its not diſcolouring the blue paper, the precipitate formed by the addition of fixed alkali, and the white colour given to the copper; and the nitrous fumes produced by the addition of oil of vitriol, as well as the manner in which paper impregnated with the reſiduum burnt, ſhew as plainly, that the acid produced was of the nitrous kind. It is remarkable, however, that during this experiment there were no ſigns which ſhewed when the ſoap-lees became ſaturated. The only time when the diminution proceeded much ſlower than uſual was on January 4. It then ſeemed to go on very ſlowly; but as the air abſorbed at that time was only 4830 meaſures, which is much leſs than what ſeems requiſite to ſaturate the alkali, and as the diminution immediately went on again upon adding more common air, it ſeems not likely, that the ſoap-lees were ſaturated at that time.

On January 10, Mr. GILPIN obſerved a ſmall quantity of whitiſh ſediment on the ſurface of the mercury; which ſeems

to

to ſhew, that the ſoap-lees were then ſaturated, and that the acid was beginning to corrode the mercury. The quantity of air abſorbed was alſo 6840 meaſures, which is about as much as I expected would be required. However, as I was perſuaded, from the event of my own experiments, that the diminution would either intirely ceaſe, or go on very ſlowly, as ſoon as the ſoap-lees were ſaturated; and as I was unwilling to ſtop the experiments before that happened, I thought it beſt to continue the electrification.

On the ſame morning Mr. GILPIN found, that about 120 meaſures of the air in the bent tube had been ſpontaneouſly abſorbed during the night, the quantity therein being ſo much leſs than it was the preceding evening, though the electrical machine had not been worked, or any thing done to it during the intermediate time. The reaſon of this in all probability is, that as the acid was then corroding the mercury, the ſoap-lees became impregnated with nitrous air, which, during the night, united to the dephlogiſticated air, and cauſed the diminution.

Though in reality the event of this experiment was ſuch as to eſtabliſh the truth of my poſition, that the mixture of dephlogiſticated and phlogiſticated air is converted by the electric ſpark into nitrous acid, as fully as if the experiment had been ſtopped in proper time; yet, as the event was in ſome meaſure different from that of my own experiments, and might afford room for cavil, I was deſirous of having it repeated; and as Mr. GILPIN was ſo obliging as to undertake it again, the materials were, on February 11. put together for a freſh experiment, in the preſence of moſt of the above-mentioned Gentlemen. The ſoap-lees employed were the ſame as before, but 183 meaſures were now introduced. The dephlogiſticated air was

different,

different, the former parcel being all uſed. It was prepared, like the former, from turbith mineral, but was rather purer, as it ſeemed to contain only $\frac{1}{12}$ of phlogiſticated air. The proportion in which it was mixed with common air was that of 22 to 10; ſo that a greater proportion of common air was now uſed, in conſequence of which it was not neceſſary for Mr. GILPIN to introduce common air ſo often.

On February 29, the reſervoir was again filled with air of the ſame kind, in preſence of ſome of the ſame Gentlemen. As it was found by the laſt experiment that we muſt not depend on the ſaturation of the ſoap-lees being made known by any alteration in the rate of diminution, the proceſs was ſtopped as ſoon as the air abſorbed was ſuch as from my own experiments I judged ſufficient to neutralize the ſoap-lees. This was effected on the 15th of March. The air remaining in the tube, when Mr. GILPIN left off working, was 600 meaſures; but at the time the produce was examined, it was reduced to about 120, ſo much having been abſorbed without the help of any electrification, which is a ſtill more remarkable inſtance of ſpontaneous abſorption than what occurred in the former experiment. A few days after the experiment began, a black film was formed in one of the legs, which, I ſuppoſe, muſt have been a mercurial *ethiops*; but whether owing to ſome ſmall degree of foulneſs in the mercury or tube, or to any other cauſe, I cannot tell. This foulneſs ſeemed not to increaſe; but on March 10, when the air abſorbed was about 5200, a whitiſh ſediment began to appear on the ſurface of the mercury.

On March 19, the produce was examined in the preſence of Dr. BLAGDEN, Dr. DOLLFUSS, Dr. FORDYCE, Dr. HEBERDEN, Dr. J. HUNTER, Mr. MACIE, and Dr. WATSON.

The mixed air forced into the bent tube from the reſervoir was 6650 meaſures, beſides which Mr. GILPIN had at different times introduced 630 of common air, which makes in all 7280, containing 4570 of dephlogiſticated, and 2710 of common air.

The ſoap-lees were evaporated to dryneſs as before. The reſiduum weighed two grains, but there were two or three globules of mercury mixed with it, which might very likely weigh half a grain. This being diſſolved in a ſmall quantity of water, the following experiments were made with it.

It did not at all diſcolour paper tinged with blue flowers.

Slips of paper were dipped into it, and dried; and, by way of compariſon, other ſlips of paper were dipped into a ſolution both of common nitre and phlogiſticated nitre, and alſo dried. The former burnt in the ſame manner, and with as ſtrong marks of deflagration, as the latter.

It had a ſtrong taſte of nitre, but left alſo a ſlight metallic taſte on the tongue.

It did not give any white colour to a piece of clean copper put into it.

In order to ſee whether the whitiſh ſediment, which was before ſaid to be formed in the bent tube, contained any mercury, the remainder of this ſolution was diluted with ſome more diſtilled water, and ſuffered to ſtand till the white ſediment had ſubſided. The clear liquor being then poured off, the remainder, containing the ſediment, which ſeemed to amount only to a very ſmall quantity, was put on a piece of bright copper, and dried upon it; a piece of clean gold was then laid over it, and both were expoſed to heat. Both metals acquired a whitiſh colour, eſpecially the gold, but which was very indeterminate.

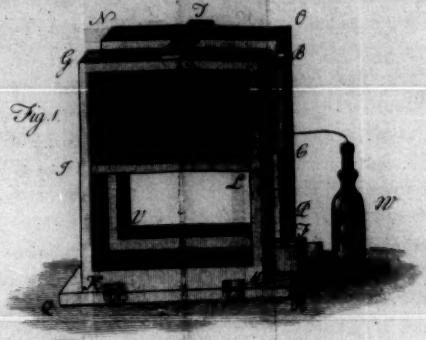


Fig. 1.

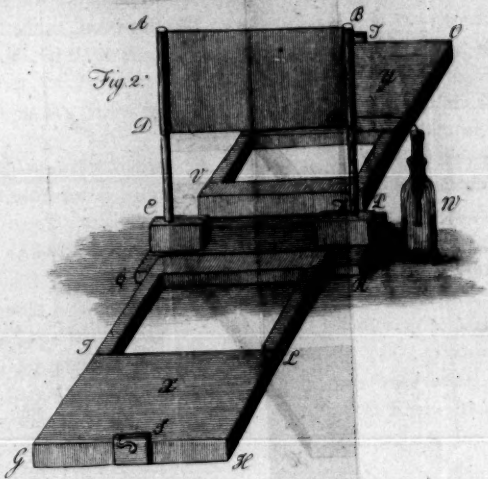


Fig. 2.

In order to diſcover how nice a teſt of alcalinity the paper tinged with blue flowers was, a ſaturated ſolution of common nitre was mixed with $\frac{1}{110}$ of its bulk of the ſoap-lees; and this mixture was found to turn the paper evidently green; ſo that, as the ſolution of nitre contains about twice as much alkali as the ſoap-lees, it appears, that if the reſiduum had wanted only $\frac{1}{220}$ part of being ſaturated, it would have diſcoloured the paper.

From the foregoing trials it appears, that the mixture of dephlogiſticated and common air in this experiment was actually converted into nitrous acid, and was ſufficient not only to ſaturate the ſoap-lees, but alſo to diſſolve ſome of the mercury. The quantity diſſolved, however, was very ſmall, and not ſufficient to diminiſh ſenſibly the deflagrating quality of the nitre; ſo that the proof of the air being converted into nitrous acid was as evident as if no mercury had been diſſolved.

In this experiment, as well as the former, no indication of the ſoap-lees becoming ſaturated was afforded by any ceſſation in the diminution of the air; whereas, in my experiments, it was very manifeſt. I do not know what this difference ſhould be owing to, except to Mr. GILPIN's giving much ſtronger electrical ſparks than I did. In his experiments the metallic knob which received the ſpark, and conveyed it to the bent tube, was uſually placed at about 2¼ inches from the conductor, ſo that the ſpark jumped through 2¼ inches of air, in paſſing from the conductor to the knob, beſides from 1¼ to 2¼ inches of air in the tube; whereas in my experiments, I believe, the knob was never placed at the diſtance of more than 1¼ inch from the conductor, and the quantity of air in the tube was much leſs; but the conductor and electrical machine were the ſame.

Except this, the only difference I know in the manner of conducting the experiment is, firſt, that Mr. GILPIN uſually continued working the machine for half an hour at a time, whereas I ſeldom worked it more than ten minutes; and, ſecondly, that in Mr. GILPIN's Experiments the common air in the reſervoir bore a leſs proportion to the dephlogiſticated air than in mine; in conſequence of which it was neceſſary for him frequently to introduce common air. On this account, the proportion of the two airs in the bent tube would be conſiderably different at different times; but on the whole, the common air abſorbed bore a greater proportion to the dephlogiſticated than in mine.

Though the whole quantity of air abſorbed in theſe experiments is known with conſiderable preciſion, yet it is impoſſible to determine, with any accuracy, how much of each kind was abſorbed, on account of our uncertainty about the nature of the air which remained at the end of the experiment. But if in the laſt experiment we ſuppoſe that the air abſorbed ſpontaneouſly between the 15th and 19th of March was intirely dephlogiſticated, and that what remained at the end of that time was of the purity of common air, it will appear, that 4090 of dephlogiſticated and 2588 of common air, which is equivalent to 4480 of pure dephlogiſticated air and 2198 of phlogiſticated air, were abſorbed at the time the electrification was ſtopped, and conſequently the dephlogiſticated air is $\frac{204}{100}$ of the phlogiſticated air; whereas in my firſt experiment it ſeemed to be $\frac{330}{100}$, and in my laſt $\frac{111}{100}$.

But the quantity of acid produced, and conſequently, I ſuppoſe, the ſaturation of the ſoap-lees, depends only on the quantity of phlogiſticated air abſorbed; and the effect of the greater or leſs quantity of dephlogiſticated air is only to make the nitre

nitre produced more or leſs phlogiſticated. Now, in this experiment, the bulk of the phlogiſticated air was $12\frac{5}{10}$ that of the ſoap-lees. In my firſt experiment it was $11\frac{9}{10}$, and in my laſt $10\frac{5}{10}$.

As many perſons ſeem to have ſuppoſed that the diminution of the air in theſe experiments is much quicker than it really is, though I do not know any thing in my Paper which ſhould lead to ſuppoſe that it was not very ſlow, it may be proper to ſay ſomething on this head. As the quickneſs of the diminution depends ſo much on the power of the electrical machine, I can only ſpeak as to what happens with the machine uſed in theſe experiments. This was one of Mr. NAIRNE's patent machines, the cylinder of which is $12\frac{1}{4}$ inches long, and 7 in diameter. A conductor of 5 feet long, and 6 inches in diameter, was adapted to it, and the ball which received the ſpark was placed at two or three inches from another ball, fixed to the end of the conductor. Now, when the machine worked well, Mr. GILPIN ſuppoſes he got about two or three hundred ſparks a minute, and the diminution of the air during the half hour which he continued working at a time, varied in general from 40 to 120 meaſures, but was uſually greateſt when there was moſt air in the tube, provided the quantity was not ſo great as to prevent the ſpark from paſſing readily.

The only perſons I know of, who have endeavoured to repeat this experiment, are, M. VAN MARUM, aſſiſted by M. PAETS VAN TROOTSWYK; M. LAVOISIER, in conjunction with M. HASSENFRATZ; and M. MONGE. I am not acquainted with the method which the three latter Gentlemen employed, and am at a loſs to conceive what could prevent ſuch able philoſophers from ſucceeding, except want of patience. But M. VAN MARUM, in his *Premiere Continuation des Expériences, faites*

faites par le moyen de la Machine électrique Teylerienne, p. 182. has deſcribed the method employed by him and M. VAN TROOSTWYK. They uſed a glaſs tube, the upper end of which was ſtopped by cork, through which an iron wire was paſſed, and ſecured by cement, and the lower end was immerſed into mercury; ſo that the electric ſpark paſſed from the iron wire to the ſoap-lees. After ſo much of a mixture of five parts of dephlogiſticated and three of common air as was equal to twenty-one times the bulk of the ſoap-lees * was abſorbed, ſome paper was moiſtened with the alkali, which by its burning appeared to contain nitre, but ſhewed that the alkali was not near ſaturated. The experiment was then continued with the ſame ſoap-lees till more of the air, equal to fifty-ſix times the bulk of the ſoap-lees, was abſorbed, which is near double the quantity required to ſaturate them; but yet the diminution went on as faſt as ever. It was then tried, by the burning of paper dipped into them, how nearly they were ſaturated; but they ſtill ſeemed far from being ſo.

The circumſtance of uſing the iron wire appears evidently objectionable, on account of the danger of the iron wire being calcined by the electric ſpark, and abſorbing the dephlogiſticated air; and when I firſt read the account, I thought this the moſt probable cauſe of the difference in the reſult of our experiments; but I am now inclined to think that the caſe was otherwiſe. From the manner in which M. VAN MARUM expreſſes himſelf, it ſeems that the only circumſtance, from which they concluded that the alkali was not ſaturated, was the imperfect marks of deflagration, that the paper dipped into it exhibited in burning; which, as we have ſeen, might proceed as well from ſome of the mercury having been diſſolved

* This is rather more than half of that requiſite to ſaturate the ſoap-lees.

as

as from the alkali not being ſaturated. I am much inclined to think, therefore, that, ſo far from the ſoap-lees not having been ſaturated, the quantity of acid produced was in reality much more than ſufficient for this purpoſe, and had diſſolved a good deal of the mercury; for the quantity of air abſorbed favours this opinion, and the phænomena agree well with Mr. GILPIN's firſt experiment, in which this was certainly the caſe; whereas, if the diminution had proceeded chiefly from the dephlogiſticated air being abſorbed by the iron, the tube towards the end of the experiment would have been filled chiefly with phlogiſticated air, which would have made the diminution proceed much ſlower than before; but we are told, that it went on as faſt as ever. It is moſt likely, therefore, that the apparent diſagreement between their experiment and mine proceeded only from their having continued the proceſs too long, and from their not having properly examined the produce.

M. VAN MARUM then proceeds to ſay: "Surpris de cette "différence de réſultat j'envoyai une deſcription exacte de nos "expériences à M. CAVENDISH, le priant en même tems de m'in- "ſtruire s'il pourroit trouver la cauſe de cette différence; et "comme la ſeule différence eſſentielle, par laquelle notre expé- "rience différoit de celle de M. CAVENDISH, conſiſtoit en ce que "nous avons employé de l'air pur produit du précipité rouge ou "du minium, au lieu de l'air pur produit de la poudre noire "formée par l'agitation du mercure avec le plomb, dont M. "CAVENDISH ne donne pas la maniere de le produire *, je le "pria;

* The uſing the iron wire formed a material difference in our manner of conducting the experiment, and one which may, perhaps, have had great influence on the reſult; but I do not ſee how the uſing ſome other kind of dephlo-

" priai de me communiquer de quelle maniere il étoit venu a " cet air, parceque je desirois de répéter l'expérience avec ce " même air: mais comme il ne m'a fourni aucune élucidation " sur la cause vraisemblable de la différence du resultat de nos " expériences, et qu'il ne lui a pas plu de me communiquer sa " maniere de produire l'air pur qu'il avoit employé pour les ex- " périences, m'écrivant, qu'il s'étoit proposé d'en parler dans un " écrit public, la longueur ennuyante de ces expériences nous " a fait prendre la resolution de différer leur continuation, pour " obtenir une parfaite saturation de la lessive, jusqu'à ce que M. " CAVENDISH ait publié sa maniere de produire l'air pur, dont il " s'est servi, nous contentant pour le present d'avoir vu, que " l'union du principe d'air pur et de la mofette produit de l'acide " nitreux, suivant la découverte de M. CAVENDISH."

As I should be sorry to be thought to have refused any necessary information to a Gentleman who was desirous to repeat one of my experiments, and who by his situation was able to do it with less trouble than any one else, I hope the Society will indulge me in adding a copy of my answer, that they may judge whether this is in any degree a fair representation of it.

" TO M. VAN MARUM.

" SIR,

" I received the honour of your letter, in which you inform " me of your ill success in trying my experiment on the con-

dephlogisticated air, instead of that prepared from Dr. PRIESTLEY's black powder, can in the least degree form an essential difference, as in the same paragraph in which I mention my having used this kind of air in my first experiment, I say, that in my second experiment I used air prepared from turbith mineral.

" version

" verſion of air into nitrous acid by the electric ſpark. It is " very difficult to gueſs why an experiment does not ſucceed, " unleſs one is preſent and ſees it tried; but if you intend to " repeat the experiment, your beſt way will be to try it with " the ſame kind of apparatus that I deſcribed in that Paper. " If you do ſo, and obſerve the precautions there mentioned, I " flatter myſelf you will find it ſucceed. The apparatus you " uſed ſeems objectionable, on account of the danger of the " iron being corroded by abſorbing the dephlogiſticated air."

" As to the dephlogiſticated air procured from the black " powder formed by agitating mercury mixed with lead, as " it was foreign to the ſubject of the Paper, and as I propoſed " to ſpeak of it in another place, I did not deſcribe my me- " thod of procuring it. As far as I can perceive, the ſucceſs " depends intirely on carefully avoiding every thing by which " the powder can abſorb fixed air, or become mixed with par- " ticles of an animal or vegetable nature, or any other inflam- " mable matter: for which reaſon care ſhould be taken not to " change the air in the bottle in which the mercury is ſhaken, " by breathing into it, as Dr. Priestley did, or even by " blowing into it with a bellows, as thereby ſome of the duſt " from the bellows may be blown into it. The method which " I uſed to change the air was, to ſuck it out by means of an " air-pump, through a tube which entered into the bottle, " and did not fill up the mouth ſo cloſe but what air could " enter in from without, to ſupply the place of that drawn " out through the tube.

" I am, &c."

With regard to the main experiment, it was not in my power to give him further information than I did; as I pointed out

the only circumstance to which, at that time, I could attribute the difference in our results. And with regard to the manner of preparing the dephlogisticated air from the black powder, I have mentioned all the particulars in which my manner of proceeding differed from Dr. PRIESTLEY's, and have also explained on what I imagine the success intirely depends; so that, I believe, no one at all conversant in this kind of experiments will think that I did not communicate to him my method of procuring that air.

XVIII. *Experiments on the Effect of various Subſtances in lowering the Point of Congelation in Water.* By Charles Blagden, *M. D. Sec. R. S. and F. A. S.*

Read April 24, 1788.

THE experiments which were neceſſary, in order to determine what effect the admixture of various ſubſtances would produce upon the property of water to be cooled below the freezing point*, naturally led me to a more particular conſideration of the power of ſuch admixtures in making water require a greater degree of cold before it congeals. Many curious queſtions occurred on this ſubject, which could only be anſwered by freſh experiments. Theſe were made nearly in the ſame manner as the preceding; that is, the liquor, whoſe freezing point I meant to try, was put into a glaſs tumbler, to the height of two or three inches above the bottom, and the tumbler was then immerſed in a frigorific mixture of common ſalt and ice or ſnow. As the object was ſeldom to cool the liquor as much as poſſible below its freezing point, the operation required no particular care, but the frigorific mixture was commonly permitted to act with its full force: wherefore, ſome parts of the liquor were often much colder than others; and particularly, near the top, a thermometer would uſually ſtand ſeveral degrees higher than toward the bottom, becauſe the ſurface was conſtantly warmed by the air of the room.

* See p. 125. of this Volume.

My first object of investigation was the ratio according to which equal additions of the same substance depress the freezing point. I began with common salt; and in order to avoid as much as possible a fallacy in the experiments, because the ordinary salt is never pure, and often not perfectly neutral, I chose some clean salt, in fair crystals, which is sold in London under the name of Borrowstounness pure salt. This salt I dissolved in distilled water, in various proportions, and found the corresponding points of congelation to be as is expressed in the annexed table; where the first column indicates the number of parts and decimals of water to one part of the salt, and the second column shews the freezing point found by the experiment. It appeared clearly, on comparing the proportions of water to salt, with the corresponding number of degrees which the freezing point was reduced below 32°, that the effect of the salt was nearly in a simple ratio; namely, that if the addition of a *tenth* part of salt to the water sunk the freezing point about 11 degrees, or to 21°, it would be depressed double that quantity, or to 10° nearly, when a *fifth* part of salt was dissolved in the water. To shew, therefore, how far this simple proportion is exact, I have added a third column to the table, which is made by selecting the lowest freezing point that was obtained without ambiguity in the experiment, and calculating, by a simple inverse proportion, what all the other points should have been according to that ratio. Thus when a fourth part of its weight of common salt was dissolved in water, I found the freezing point of the liquor to be 4°; therefore, to determine what it should be when only $\frac{1}{11}$ part of salt was added to the water, the formula is 32 : 4 :: 28 (the number of degrees that the point 4° is below the freezing point of pure water) : $3\frac{1}{2}$; which subtracted from 32° gives $28°\frac{1}{2}$

28°¼ for the freezing point of that ſolution. All the reſt of the third column of the table is found in the ſame manner, and with very little trouble, becauſe $4 \times 28 = 112$ is a conſtant number, which being divided by the numbers of the firſt column, the quotient is the number of degrees ſought. In all the experiments, none but diſtilled water was employed.

Common Salt.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
32 : 1	29	28¼
32 : 1	28+	28¼
24 : 1	27½	27½
16 : 1	25½	25
10 : 1	21½	20½
7,8 : 1	18½	17½
6,2 : 1	13½	14
5 : 1	9½	9½
5,5 : 1	7	7
4 : 1	4	4

The third column is calculated from the laſt experiment, in which the freezing point of a ſolution of one part of ſalt in four of water proved to be +4°.

The numbers in the third column of the table come ſo near to thoſe in the ſecond, that moſt likely the ſmall differences between them ought to be aſcribed to errors in the experiment; whence we ſhould conclude, that the ſalt lowers the freezing point in the ſimple inverſe ratio of the proportion which the water bears to it in the ſolution.

In the courſe of the experiments made with the different ſolutions of this ſalt, ſome phænomena occurred which deſerve further attention.

1. This ſolution was in one inſtance cooled 8¼, and in ſeveral 5 or 6 degrees, below its freezing point; but in general it

ſhot rather more readily than ſome other ſolutions; which I aſcribed, from the analogy of my former experiments, to its leſs tranſparency. Though the common ſalt did not form a turbid ſolution, yet there was ſomething of a greyiſh caſt, much like what is produced by the mixture of a ſmall portion of clay with pure water; and the caution in conducting the operation, neceſſary to make ſolutions of this ſalt cool a certain number of degrees below their freezing point, ſeemed to be nearly the ſame as would be required with water equally tinged by means of clay.

2. When the ſolution was very ſtrong, nearly ſaturated with the ſalt, cooling it tended ſomewhat to leſſen this greyiſh caſt, to render it rather more limpid: as was particularly the caſe in a ſaturated ſolution which I cooled to 0°. I ſuſpect this effect takes place when the ſolution begins to expand with the cold.

3. In the ſtrong ſolutions, when the proportion of ſalt was as much as one-fifth, before the real congelation came on which reduced the whole to one temperature, ſeveral ſmall ſtellated cryſtals formed in the liquor, which increaſed very ſlowly. Theſe were real ice of the ſalt water; but the cold being in this caſe very little below the freezing point of the ſolution, there was little power to make them ſpread; and ſometimes, when this occurred, I believe, part of the ſolution toward the top was rather above than below the freezing point. Similar phænomena were obſerved, and in a ſtill more remarkable manner, in the ſolutions of ſome other ſalts, always when they were very ſtrong: and it may be given as a general fact, that in ſolutions approaching to ſaturation, though the formation of one glacial cryſtal does, as in pure water, tend to make the whole freeze and come up to an uniform temperature, yet the ſhooting goes on much more ſlowly, as if the

ſalt oppoſed ſome reſiſtance, or at leaſt proved an impediment, to the progreſs of the cryſtallization. By ſtirring the ſolution, however, when theſe ſtellated cryſtals once began to form, they quickly increaſed in bulk, or became more numerous, and the liquor aſſumed its freezing temperature.

4. This ſalt with ſnow, in the manner of frigorific mixtures, produced a cold of $-4°$.

The next ſalt which I tried for its effect in lowering the freezing point of water, was nitre. It was part of a large compound cryſtal, or bundle of cryſtals, apparently very pure, ſuch as is uſed in manufacturing the beſt gunpowder. This being mixed with the diſtilled water, in different proportions, the ſolutions froze according to the following table.

Nitre.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.	
	°	°	
32 : 1	30¼	30¼	
24 : 1	30	30	The third column is calculated from the fifth experiment, in which the freezing point of a ſolution of one part of ſalt in eight of water proved to be 26°.
16 : 1	28¾	29	
10 : 1	27	27¼	
8 : 1	26	26	
7,9 : 1	26¼	ſalt depoſited	
7 : 1	26¼	ſalt depoſited	
6,85 : 1	27	much ſalt depoſited.	

Nitre is well known to differ from common ſalt in being much more ſoluble in warm than in cold water. Hence it would be nothing remarkable, that the ſolutions being made in

water

water above the freezing point, ſome of the ſalt ſhould, when they exceeded a certain ſtrength, be depoſited before they began to freeze. But a further queſtion occurred here, whether, when a ſolution was cooled below its freezing point, the ſalt would ſtill continue to be depoſited; or whether it would not have parted with all the ſalt it was obliged to let go by the time it came to the degree at which it was to freeze, and would retain the remainder notwithſtanding any ſubſequent cooling. To determine this, I noticed carefully the quantities of ſalt depoſited at the bottom of the tumbler, in compariſon with the cold of the ſolution as ſhewn by the immerſed thermometer; and I found, that in ſome caſes (for inſtance, when the ſalt was to the water only as 1 : 10) the depoſition did not begin till after the ſolution had paſſed its freezing point; and that, when it began earlier, ſtill there was no ſtop at the freezing point, but the quantity continued augmenting as the cold of the ſolution proceeded, and, as far as I could judge, rather in an increaſing ratio. Thus when the ſaturated ſolution was cooled 8 or 10 degrees below its freezing point, which often happened, the collection of nitre at the bottom was very great; and in this manner I could render a ſaturated ſolution of nitre no longer ſaturated when it came to freeze, the deficiency being ſometimes ſo great as to raiſe the point of congelation a degree or more. Hence was aſcertained the unexpected fact, that the lower ſuch ſolutions are cooled, the higher is their freezing point.

The nitre depoſited by the ſolution as it cooled, formed, if the veſſel remained at reſt, ſmall but very white and compact priſmatic or needle-like cryſtals, of conſiderable length, pointing different ways, and at laſt curiouſly interwoven with one another. But if theſe were broken down, or the ſolution was

ſtirred

ſtirred with any force, the remaining nitre depoſited itſelf in ſuch minute cryſtals as to have much the appearance of a powder; I ſuppoſe from the deſtruction of the regular ſurfaces upon which it would otherwiſe have continued to form.

Frequently, in the ſtronger ſolutions, there appeared near the bottom and ſide of the tumbler many elegant ſtellated cryſtals, perhaps a quarter of an inch in diameter, all ſeparate, but ſometimes crowding very cloſe upon one another, ſo as to exhibit a ſpectacle of much beauty.

The ice of ſolutions of nitre, eſpecially when it began to thaw, was very different from common ice, having a ſoft woolly appearance, as if of a more tender and looſe texture. Something of the ſame kind was obſervable in the ice of all the other ſolutions, ſufficiently diſtinguiſhing it from any that can be formed of pure water.

All the ſolutions of nitre were remarkably limpid, having no tendency to an opaque or turbid caſt; and accordingly they were very eaſily cooled below the freezing point, and could not but with difficulty be made to ſhoot till they had paſſed it many degrees. In two inſtances they cooled more than 10 degrees; namely, a ſolution of one part of nitre in 24 of water cooled ſlowly to 19°¼, and then ſhooting, the thermometer came up to 30°; and another ſolution, in which the nitre was to the water as 1 : 10, cooled rather below 16°, and having produced ſome ſtellated cryſtals, roſe, when the perfect congelation took place, up to 27°.

As, when pure water is cooled below its freezing point, the leaſt particle of ice or ſnow brought into contact with it cauſes an inſtant congelation, I was curious to know whether the ſame effect would be produced when ſalts were diſſolved in the water. Therefore, having one of theſe nitrous ſolutions, whoſe

proportions were 8 : 1, I cooled it to 24°, about two degrees below its freezing point, and then, no ſalt being depoſited, I put into it a ſmall bit of ice. The effect of this was not inſtantaneous, as in pure water, though ultimately the ſame; the bit of ice gradually enlarged, and when it was ſtirred about in the liquor, a number of ſtar-like cryſtals formed, which being ſcattered through it ſoon brought it to an uniform temperature of 26°. This ſame ſolution, when cooled in a preceding experiment to 18°, had its freezing point at 27°, from the quantity of nitre that had been depoſited. In all ſolutions, therefore, of ſuch ſalts as are much more ſoluble in hot than in cold water, if it be deſired to find their freezing point when they are loaded with as much of the ſalt as the water can contain at that temperature, the moſt effectual method is to oblige them to ſhoot, as ſoon as they can be made to do ſo, by putting in a ſmall bit of ice or ſnow; for thus the fallacy which might otherwiſe ariſe from the depoſition of ſome of the ſalt will be avoided.

A doubt having been ſuggeſted, whether the contact of a cryſtal of ſalt might not alſo bring on the congelation, that experiment was tried, but it produced no effect. Indeed, the formation of ſaline cryſtals in theſe experiments, the liquor ſtill remaining fluid, was a ſufficient proof to the contrary.

There is another phænomenon which I thought took place in ſolutions of nitre and ſome other ſalts, but could not abſolutely ſatisfy myſelf concerning it; namely, that the depoſition of ſome ſalt haſtened that of the remainder. For inſtance, ſuppoſe that a ſolution of nitre in the proportion of 1 : 8, would retain the whole of its ſalt at its freezing point of 26°; then, if another ſolution were made much ſtronger, it would begin to depoſit its ſalt before it came to 26°; and when cooled to 26°,

26°, it would not have juſt loſt the difference between the proportions of 6 : 1 and 8 : 1, but would have depoſited more of the ſalt, ſo that now at 26° it would contain perhaps only a tenth of nitre; as if the formation or exiſtence of ſome cryſtals in the ſolution facilitated the ſeparation of the ſalt, or conſtituted an attractive power counteracting the attraction of the ſalt to the water. It would have required a particular ſeries of experiments to aſcertain this as a fact; but ſeveral obſervations incline me to believe it is ſo; and the conſequences to be drawn from it would be ſufficiently curious, and applicable to many points of theory. Of the ſame nature, perhaps, is another circumſtance already hinted at, that after the ſolution has begun to form cryſtals of nitre, the depoſition proceeds with increaſed rapidity as the cooling goes on. This may, indeed, be owing to the attraction of the water to the ſalt decreaſing in a progreſſive ratio with each degree of cold; but it may alſo depend on the attracting ſurfaces being multiplied as more cryſtals are formed: and ſtirring the liquor ſeems to haſten the progreſs of the depoſition; which may be aſcribed to its bringing freſh portions of the ſolution perpetually to thoſe attracting ſurfaces; for, if there be ſuch an attraction, it can ſcarcely be ſuppoſed to take place at any ſenſible diſtance.

Upon the whole it ſeems evident from the preceding table, that the effect of nitre, like that of common ſalt, is to depreſs the freezing point in the ſimple ratio of its proportion to the water; which will be found univerſally true when allowance is made for the depoſition and other ſources of fallacy already enumerated.

This nitre produced, with ſnow, a cold of between 26° and 27°.

As nitre ſunk the freezing point of water ſo little, namely, but 6 degrees, I had recourſe for the next ſet of experiments to that neutral ſalt which, after ſea ſalt, produces the greateſt cold with ice; which is, the common ſal ammoniac. The different ſolutions of this ſalt in water, being ſubmitted to the action of the frigorific mixtures, froze according to the following table.

Sal ammoniac.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.	
	°	°	
15,7 : 1	24½	24½	The third column is calculated from the laſt experiment but one, in which the freezing point of a ſolution of one part of the ſal ammoniac in five of water proved to be 8°.
10 : 1	20½	20	
9,8 : 1	20	19½	
7,9 : 1	16½	16½	
6 : 1	12	12	
5 : 1	8	8	
4 : 1	4	ſalt depoſited	

In this table alſo the numbers of the third colum agree ſufficiently with thoſe of the ſecond to ſhew, that ſal ammoniac, like the two preceding ſalts, depreſſes the freezing point in the ſimple ratio of the proportion in which it is mixed with the water.

The following is the account, as written at the time, of the experiment where the ſalt was to the water as 1 : 5. "This "ſolution cooled down to 2° at bottom, but the frigorific mix- "ture being inſufficient, the top remained at 10°. The tum- "bler was then taken out, and the frigorific mixture being "refreſhed by ſtirring and the addition of new materials, it

"was

" was replaced, when ice ſoon began to form at the bottom, " like feathered cryſtals, which however ſhot out very ſlowly. " But upon ſtirring the ſolution, a vaſt number of ſtellated " feathered cryſtals preſently formed in it, the thermometer " roſe immediately to 7°¼, and was ſoon got to 8°." I have tranſcribed theſe remarks, not only becauſe they elucidate ſome of the phænomena mentioned in former experiments, but likewiſe on account of the ſtriking analogy the cryſtals bore to the configurations obſerved in a drop of the ſolution of this ſalt viewed with the microſcope. As the drop dries, great numbers of feathered cryſtals ſhoot out from its edges, and ſome more perfect ſtellated cryſtals are often formed in the middle; of which a pretty exact repreſentation may be ſeen in BAKER's Employment for the Microſcope (p. 110. Pl. II.). The reſemblance of both kinds of cryſtals, in theſe two different ſituations, is very great; and their particular figure is probably due to the ſame cauſe, a confuſed and conſtrained cryſtallization, occaſioned in the one caſe by the haſty evaporation and want of room in the drop, and in the other caſe by the watery congelation acting upon and embarraſſing the natural tendency of the ſalt.

A ſtronger ſolution of ſal ammoniac, the proportions being 1 : 4, exhibited a ſtill more beautiful appearance; for, beſide the feathered cryſtals at bottom, and the ſtars floating in the middle, a third ſort of cryſtals, moſt reſembling the ſtars, attached themſelves to the ſides of the tumbler, and were ſeen through the glaſs as compoſing a great variety of brilliant *facettes*. In this experiment the ſolution was reduced below 1°, before any congelation began to take place, and a certain quantity of the ſalt fell to the bottom in the form of a white powder, about as much as reduced the liquor to the ſtate of a

ſaturated

ſaturated ſolution at the freezing point, which therefore, when the congelation took place, proved to be 4°, inſtead of 2°, the number that would have anſwered to this proportion of ſalt.

It has been a queſtion much conteſted, whether ſaline ſolutions depoſit their ſalt upon freezing. That ſome ſeparation, or a tendency to ſeparation, takes place, many facts concur to prove; and among the reſt ſome phænomena obſerved in the above-mentioned experiments. For inſtance, the ſtellated cryſtals, when firſt formed, were barely ſuſpended in the water, and ſometimes they even gradually ſubſided to the bottom; which ſhews, that they conſiſted of ſalt chiefly, only inviſcated with ice, or at leaſt of an over-proportion of ſalt: for the principal maſs of ice formed in a ſaturated ſolution floats in it like common ice in pure water. Beſides, it was almoſt conſtantly found, that when a cruſt of ice, which had been formed in a ſaline ſolution, began to thaw, a thermometer thruſt into it roſe ſomewhat higher than the point at which it had become ſtationary when the congelation took place; an indication that a leſs proportion of ſalt was preſent in it than had been in the whole ſolution. And if, after any ſolution had ſhot and formed a quantity of ice, the tumbler were ſtill kept in the frigorific mixture, the thermometer immerſed in the liquor gradually ſunk lower and lower, as the congelation proceeded. But theſe very obſervations ſhew, that the ſeparation is far from perfect; as the riſe of the thermometer in the former caſe, or its deſcent in the latter, never exceeded one or two degrees, under a great variation in the quantity of ice in the ſolution.

Sometimes in ſolutions of ſal ammoniac, and ſuch other ſalts as ſeparate by the cooling of the water, a ſort of flocculent ſubſtance is formed, which ſubſides in the water, and thereby

thereby is diſtinguiſhed from the proper ice of the ſolution, which it otherwiſe much reſembles in appearance. It is compoſed principally of the depoſited ſalt, in very minute cryſtals like powder, inviſcated and kept together with a little ice.

Though ſal ammoniac forms a tolerably tranſparent ſolution, yet I perceive that in theſe experiments it was in few inſtances cooled much below its freezing point; the moſt was nine degrees. I believe this to have been accidental, from no particular attention being now beſtowed on that object.

The ſal ammoniac, mixed with ſnow, produced a cold of from 4° to 4½ of FAHRENHEIT's ſcale.

Of all the ſolutions ſubmitted to theſe experiments, there were none more tranſparent and elegant than thoſe made with Rochelle ſalt. The water diſſolved a large proportion of this ſubſtance, and had its freezing point ſunk according to the following table.

Rochelle ſalt.

Proportion of water to the ſalt.	Freezing point by the experiments.	Freezing point by calculation.	
	°	°	
10 : 1	29½	29½	The third column is calcu-
5 : 1	27½	27½	lated from the laſt experi-
4 : 1	26½	26½	ment but one, in which
2,6 : 1	24	23½	the freezing point of a ſolu-
2,25 : 1	22½	22½	tion of one part of the
2 : 1	21	21	Rochelle ſalt in two parts of
1,6 : 1	24	ſalt depoſited	water proved to be 21°.

All the ſolutions of Rochelle ſalt bore to be cooled remarkably well. In one inſtance the liquor ſunk 11½ degrees below

its freezing point; namely, the ſolution of one part of the ſalt in five of water, whoſe freezing point proved 27°½, and which cooled to 16° before the cryſtals of ice ſhot. In two other inſtances it ſunk fully nine degrees below its freezing point. And, as already has been mentioned, the ſolutions of this ſalt are remarkably tranſparent.

In trying the greateſt cold to be obtained by mixing Rochelle ſalt with ſnow, I could get the thermometer no lower than 24°.

GLAUBER's ſalt likewiſe was ſubjected to the experiments, but its utmoſt effect in producing cold with ſnow appearing to be only two degrees, this was too ſmall a ſcale for ſettling any thing as to the ratio. A ſolution of it in water, in the proportion of 1 : 5, cooled readily to 31°; but the ſalt was depoſited in great quantities, and often ſo faſt, as to ſtop the cooling of the bottom of the liquor entirely, though the veſſel was immerſed in a ſtrong frigorific mixture. This phænomenon has been obſerved before; but does not take place, to the ſame degree, in the ſolutions of any other ſalt with which I am acquainted, though I evidently perceived ſomething of it in the ſtrong ſolutions both of nitre and of ſal ammoniac. It is exactly the converſe of the cold produced by diſſolving ſalts in water; for as there ſome heat is abſorbed, and becomes latent, by the change of the ſalt from a ſolid to a fluid ſtate, ſo here ſome heat is evolved as the ſalt aſſumes the ſolid cryſtalline form. The effect is ſo much more manifeſt with GLAUBER's ſalt only, I imagine, becauſe the formation of the cryſtals proceeds ſo rapidly; whence the quantity of heat generated equals or exceeds the cold communicated by the freezing mixture. Some odd appearances are produced by this ſudden

ſtop

ſtop of the cooling, and the rapid depoſition of ſalt; for inſtance, a particular ebullition in certain parts of the liquor; but any intelligible deſcription of them would be too minute.

Theſe were all the ſalts with an alkaline baſis which I tried. They all agreed as to the chief object of theſe experiments, namely, to determine how much the freezing point of water would be ſunk by diſſolving them in it in various proportions; which by theſe experiments appears to be, as nearly as could be determined, according to the ſimple ratio of the proportion each ſalt bears to the water.

I now reſolved to try a few ſalts with an earthy and metallic baſis.

The common ſal catharticus amarus of the ſhops was the ſpecimen I uſed of an earthy ſalt. It formed a turbid inelegant ſolution, as if dirty; and with various proportions of water produced the following points of congelation.

Sal catharticus amarus.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
16 : 1	31	31
10 : 1	30	30¼
4 : 1	28½	28
3 : 1	26½	26½
2,4 : 1	25½	25½

The third column is calculated from the laſt experiment, where the freezing point of a ſolution of one part of the ſal catharticus amarus in 2,4 of water proved to be 25°½.

No ſalt was depoſited from the ſtrongeſt of theſe ſolutions; and as that which I uſed was a deliqueſcent ſalt, it muſt probably have been in a vaſt proportion to the water, before any

ſuch effect would have taken place. I have ſunk a thermometer with it and ſnow to 7°½; which, according to the proportions in the table, would make more than three parts of the ſalt to two of water. Accordingly, a large quantity of the ſalt was required to the ſnow.

No particular phænomenon was obſerved with this ſalt, except the ſingular configuration of its ice, which aſſumed the form of fungi, or of ſome kinds of lichen, with feathered ſtriæ. The ſolutions were difficult to cool much below their freezing point.

Of the ſalts with a metallic baſis, green vitriol affords one of the moſt tranſparent ſolutions in water. It ſinks the thermometer nearly to 27°½ with ſnow, and reduced the freezing point of water according to the following table.

Green vitriol.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
10 : 1	30½	31
6 : 1	30½	30½
4 : 1	29½	29½
3 : 1	28½	28½
2,4 : 1	28	28

The third column is calculated from the laſt experiment, in which the freezing point of a ſolution of one part of the green vitriol in 2,4 of water proved to be 28°.

The ice formed by theſe ſolutions aſſumed a foliaceous configuration, with a texture of penniform ſtriæ, in ſome reſpects like the appearance exhibited by a drop evaporating under a microſcope, as delineated by BAKER. Scarcely any ſalt gave the point of congelation ſo regularly in the proportion of the quantities mixed with the water, and none afforded ſolutions

which cooled more eaſily and readily below the freezing point. In two inſtances the cooling was more than 11 degrees.

Having found that white vitriol, mixed with ſnow, produced a cold of 20°, melting the ſnow remarkably faſt, I was induced to try the freezing point of its ſolutions. But though it diſſolved very readily in water, yet the liquor it formed was ſo turbid and thick, that little ſatisfaction could be derived from the experiments. The only numbers to be relied upon are the following, which agree ſufficiently with the general reſult.

White vitriol.

Proportion of water to the ſalt.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
10 : 1	31	31
5 : 1	29½	30
3 : 1	28¾	28¾

The third column is calculated from the laſt experiment, in which the freezing point of a ſolution of one part of the white vitriol in three of water proved to be 28°¾.

Theſe ſolutions cooled very ill, none of them having ſunk much below the freezing point, and the ſtrongeſt, which had a copious ſediment, forming a cruſt of ice at the bottom of the tumbler, before it was reduced at all below the term of congelation.

M. Achard, of Berlin, having alledged *, that borax inſtead of raiſing the boiling point of water, like other ſaline ſubſtances, very ſenſibly depreſſes it, I determined, however extraordinary the fact might appear, to try whether it had

* See Crell's Chem. Annalen, 1786, Vol I. p. 501.

any peculiar effect on the freezing point. But having made the experiment with nearly a ſaturated ſolution of borax, the thermometer when it congealed was evidently below 32°: I believe about a degree.

As a neutral or middle ſalt, which when cryſtallized is always nearly of the ſame nature, and diſſolves in a regular proportion in water, ſeemed likely to afford the moſt ſimple caſe of the effect of extraneous admixtures, it was with ſuch that I began theſe experiments. But having found that with them the ſimple ratio prevailed, I proceeded to try ſubſtances of a more variable nature, and capable of being mixed with water in almoſt any proportion; ſuch as acids, alkalies, and ardent ſpirits. A material difference in the law, which ſeemed to occur in theſe new experiments, renders it proper to defer the account of them till ſome reflections on the preceding facts, with a few additional experiments to which they gave riſe, have been premiſed.

It is now, I believe, univerſally allowed, that in frigorific mixtures the melting of the ſnow or ice is the principal cauſe of the cold produced; all that heat which muſt become latent in order to give water its fluid form being taken from the ſenſible heat of the ingredients. But as, when cryſtallized ſalts are employed for the purpoſe, theſe alſo are reduced to a liquid form, there muſt, from this circumſtance, be ſome additional cold produced, ſuch for inſtance as would be occaſioned by diſſolving the ſame ſalt in water. Suppoſe then that the latent heat of water is 150 degrees, and that ſal ammoniac, in diſſolving to ſaturation, produces ſo much cold as ſinks the whole ſolution about 20°; it is evident, that if this ſalt and ice are mixed together in ſuch proportions as juſt to melt one another, the total cold generated in the operation muſt amount to 170 degrees.

And ſo much actually is produced before the whole liquefaction is effected; and yet a mixture of theſe two ſubſtances will ſink the thermometer no lower than to 4° of FAHRENHEIT's ſcale. The conſideration of this apparent difficulty has led to the ſuppoſition, that a certain quantity of *fire* is contained in the cryſtals of the ſalt, which being diſengaged in the ſolution keeps up the mixture to a certain temperature *. But I conceive, that the phænomenon depends ſimply on the gradual liquefaction of the ingredients, a neceſſary conſequence of the cold produced. A ſaturated ſolution of ſal ammoniac freezes itſelf at 4°; therefore, when the mixture is reduced, by the liquefaction of the ingredients, to that temperature, no more of them can melt, becauſe any addition of cold would freeze what is already melted; and if the mixture, under ſuch circumſtances, were placed in an atmoſphere of its own temperature, the ingredients would remain for ever in that ſame ſtate, without any further liquefaction. But in an atmoſphere warmer than 4°, they continue to melt, more or leſs ſlowly, as the heat which is gradually communicated furniſhes what is neceſſary to become latent. This communicated ſenſible heat being immediately converted into latent, the mixture will always be kept down to the ſame temperature as long as there is a ſufficient maſs of unmelted materials; and it can ſink no lower, becauſe then the liquefaction would be ſtopped; conſequently ſuch mixtures muſt preſerve, as they have been found to do, a pretty uniform temperature, ſo as to have been formerly uſed for graduating thermometers. And the whole cold produced, or, to ſpeak properly, the whole of the heat made to diſappear, I preſume to be ultimately equal to the full quantity of latent heat belonging to the diſſolved ice and ſalt.

* DE LUC's Idées ſur la Météorologie, Tom. I. p. 201.

According

According to this explanation, the greateſt cold to be produced with any ſalt and ice or ſnow ſhould be that with which a ſaturated ſolution of the ſame ſalt freezes; which was formerly eſtabliſhed by M. DE LUC as a fact, and is in general conformable to the preceding experiments. Thus, when a ſolution of one part of ſal ammoniac in four parts of water was put into the frigorific mixture, ſome of the ſalt was depoſited, and then the ſolution on freezing raiſed the thermometer to 4°, which is exactly the cold produced by a mixture of the ſame ſalt with ſnow. Likewiſe the ſtronger ſolutions of nitre had their freezing point between 26° and 27°; thoſe of green vitriol near 28°; and ſo of moſt of the other ſalts, correſponding with the cold of their frigorific mixtures. This agreement, however, is liable to be modified by ſeveral circumſtances. For inſtance, if the ſalt be of that nature that more of it is held in ſolution in warm than in cold water, it becomes extremely difficult to have the ſolution, at the moment it freezes, exactly loaded with that quantity of ſalt which is moſt advantageous for acting upon the ice or ſnow. Too much may have been depoſited by the cooling previous to the congelation; or ſome reſiſtance having occurred to the cryſtallization of the ſalt, more of it may be retained at the moment of congelation than properly belongs to the ſolution at that temperature. The purity of the ſalts likewiſe, their particular nature as inclined to deliqueſce or to fall down in powder in the air, the rapidity of their action upon the ice or ſnow, muſt neceſſarily be taken into the account, as influencing the degree of cold they can produce.

It is an evident corollary from the above-mentioned agreement, that if the freezing point of the ſolution of a ſalt in water in any known proportion be given, its degree of ſolubility

lity may in general be aſcertained, merely by trying the greateſt cold it will produce with ſnow.

Notwithſtanding this uſual correſpondence between the greateſt quantity of a ſalt that could be diſſolved, and the greateſt degree of cold it would produce, compared with the freezing point of its ſolution, I ſaw reaſon to believe that, with ſome ſalts at leaſt, an irregularity took place in this reſpect. The Rochelle ſalt, mixed with ſnow, did not ſink the thermometer lower than 24°; yet the freezing point of a ſolution of one part of it in two of water appears, by the numbers in the table, to have been 21°. It is remarkable, however, that in the experiment with a ſtill ſtronger ſolution, where ſome of the ſalt was depoſited, the freezing point came down to 24°. Likewiſe the ſolution of white vitriol, whoſe freezing point was 28°$\frac{3}{4}$, depoſited a copious ſediment, and yet produced with ſnow a cold of 20°. This ſediment did not look like cryſtallized ſalt, but gave a turbid appearance to the whole ſolution. The moſt remarkable circumſtance of this nature, I think, occurred with the purified common ſalt; which, when mixed with ſnow, ſunk the thermometer only to −4°. By experiment I found that one part of this ſalt would diſſolve in about 2°$\frac{1}{4}$ of water. Now, if the proportions of 1 to 4 give the freezing point at +4°, as by the table; 1 to 2$\frac{1}{4}$ would, upon the general analogy, give it between −12° and −13°; that is, 8 or 9 degrees lower than the greateſt cold the ſalt would produce with ſnow. This circumſtance leads to a ſuſpicion, that however uniform the preceding tables appear, yet in reality ſomething of a decreaſing ratio exiſts, and that each ſubſequent addition of a ſalt does not depreſs the freezing point quite ſo much as the preceding. In thoſe ſalts where the whole quantity of depreſſion is but ſmall, and in the upper

part

part of the ſcale in thoſe which have a greater effect, this progreſſion may be inſenſible, or be incapable of determination by ſuch experiments as theſe, which by their nature do not admit of extreme accuracy; but it may become ſufficiently obſervable toward the laſt additions of ſuch ſalts as are ſoluble in great quantity, and ſuch as alter very much the freezing point. I never ſucceeded to freeze the ſaturated ſolution of this Borrowſtounneſs common ſalt, though it has been cooled ſeveral degrees below $-4°$ without depoſiting any of its ſalt; if the experiment were made in a cold ſufficient for the purpoſe, it would ſhew whether any irregularity takes place in the action of this ſalt upon ſnow, or whether ſuch a decreaſing ratio does exiſt. Perhaps, however, the whole may depend upon the principle alluded to formerly, that the preſence of ſome ſalt favours the depoſition of more: for when a ſalt is mixed with ſnow, it may in this point of view be conſidered as in the ſame ſtate with a ſaturated ſolution of the ſalt which has ſome of it remaining undiſſolved; and which, therefore, will not contain more than properly belongs to it at the given temperature; and the cold of ſuch a frigorific mixture ſhould be no greater than that of the freezing point of the ſaturated ſolution under thoſe circumſtances.

As it is well known that water, after it has been ſaturated with one ſalt, will take up a certain portion of another ſalt without depoſiting any of the former, I was curious to try what effect the addition of this ſecond ſalt would produce on the freezing point; and particularly whether it would depreſs the freezing point of the ſaturated ſolution the ſame number of degrees that an equal proportion of the ſame ſalt would depreſs the freezing point of water; and whether the ſame

ſimple

ſimple ratio would hold good, or any new law take place. To bring this to the teſt of experiment, I took a ſaturated ſolution of nitre, whoſe freezing point of courſe was between 26° and 27°: and adding to it the purified common ſalt in various proportions, I obtained the following reſults.

Compound ſolution of nitre and common ſalt.

Proportion of water to the nitre.	Proportion of water to the common ſalt.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
		°	°	°
	30,2 : 1	23¼	22¼	1
A ſaturated	15 : 1	20¼	19	1¼
ſolution.	10 : 1	17¼	15¼	2¼
	7,4 : 1	13¼	11¼	2
	5 : 1	5¼	4	1¼

The ſolution of nitre was one that had been made ſome time, and contained near one-ſixth part of nitre, conſequently more than the water could keep diſſolved in the temperature of the atmoſphere, which was then not much above 40°. Some of it therefore was depoſited at the bottom. This, however, the addition of a $\frac{1}{32}$ part of common ſalt ſoon enabled the water to take up, and the two ſalts formed together a clear ſolution. In the frigorific mixture, before the congelation took place, ſome ſaline cryſtals were again depoſited, which ſeemed to be chiefly, if not entirely, nitre. In the temperature of the air this depoſit was re-diſſolved; and on attending to the circumſtances more carefully, upon a repetition of the cooling, I found, that no ſalt began to be depoſited till the ſolution was reduced to 23°, when ſome needle-like cryſtals appeared, which conſequently were nitre. The ſolution cooled about a degree more, and then, having let fall much ſalt, partly as cryſtals

and partly, by ſtirring, as a powder, it was made to congeal by dropping in a bit of ſnow, and the thermometer roſe to 23°½. When more ſalt was added, to render its proportion to the water as 1 : 15, the nitre ſeemed ſtill more difficultly depoſited in the freezing mixture; and this continued to be the effect of further additions of ſalt, the depoſition of ſaline matter previous to the congelation being always leſs, in proportion as the quantity of common ſalt was greater.

It is evident from the freezing points of this compound ſolution, that the common ſalt depreſſed the freezing point of the ſolution of nitre ſomething leſs than it would have depreſſed the freezing point of water, if added to it in the ſame proportion. To ſhew this more evidently, I have added a fourth and a fifth column to the table: the fourth column is formed by taking the freezing point of the ſaturated ſolution of nitre as 26°½, and then finding how many degrees the quantity of common ſalt added would have depreſſed the freezing point of water; this number of degrees, ſubtracted from the conſtant number 26°½, gives the freezing point by calculation; namely, what it ſhould have been if the ſalt had produced the ſame effect upon the ſolution of nitre as it would upon pure water; and the difference between this and the freezing point found by the experiment gives the numbers in the fifth column. From the table it is apparent, that the deficiency of effect from the ſalt goes on increaſing to the third experiment, after which it decreaſes. Probably ſome particular law takes place, which it would require a great number of experiments to develope; but the decreaſe toward the laſt may in part be owing to the greater quantity of nitre which the water, when it began to be loaded with common ſalt, retained at the time of congelation, and which muſt have its effect in

depreſſing the freezing point. The above-mentioned circumſtance ſeems rather contradictory to an opinion which has been entertained, that when one ſalt, added to a ſaturated ſolution of another ſalt, enables it to take up more of the former ſalt, it is only becauſe the water of cryſtallization of the ſecond ſalt really adds to the quantity of the diſſolving fluid.

I next proceeded to try a ſimilar experiment with ſal ammoniac and the purified common ſalt, but with this difference, that neither ſalt ſhould be added to the water in ſuch quantity as to come near the point of ſaturation, ſuſpecting that the diminution of effect obſerved in the foregoing experiments might depend, in part at leaſt, upon this circumſtance. The ſal ammoniac, therefore, was diſſolved in water in the proportion of 1 : 10, and the correſponding point of congelation appeared by experiment to be 20°½, agreeing very well with the table of ſal ammoniac formerly given. To this ſolution was added the purified common ſalt, in proportion to the water as 1 : 15, and then as 1 : 10; the reſulting points of congelation were as is ſhewn in the following table, conſtructed in all reſpects as the immediately preceding.

Compound ſolution of ſal ammoniac and common ſalt.

Proportion of water to the ſal ammoniac.	Proportion of water to the common ſalt.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
		°	°	
10 : 1	15 : 1	12¾	13	+ ¼
10 : 1	10 : 1	9¼	9$\frac{1}{5}$	− $\frac{1}{20}$

Hence it appears, that in this compound ſolution both ſalts produced, as exactly as the experiments can be expected to

ſhew, their full effect in depreſſing the point of congelation. When the ſolutions at length froze, after cooling many degrees below the freezing point, the cryſtals ſhot in a very beautiful manner round the bulb and up the ſtem of the thermometer.

In a compound ſolution of Rochelle and common ſalt there was, however, a deficiency of effect. For the ſolution of Rochelle ſalt in the proportion of one part to four of water, having its freezing point at 26°¼; when common ſalt was diſſolved in it, in the proportion of $\frac{1}{10}$th, the freezing point appeared by experiment to be 16°¼, whereas by calculation it ſhould have been depreſſed nearly to 15°.

A compoſition of three ſalts was affected as follows:

Compound ſolution of Rochelle ſalt, common ſalt, and ſal ammoniac.

Proportion of water to the Rochelle ſalt.	Proportion of water to the common ſalt.	Proportion of water to the ſal ammoniac.	Freezing point by the experiment.	Freezing point by calculation.	Difference.
9,8 : 1	10 : 1	17 : 1	13° −	11¼°	− 1¼°

The computation is made thus: Rochelle ſalt, in the proportion of 1 : 9,8, depreſſes the freezing point 2°¼; common ſalt, in the proportion of 1 : 10, ſinks it 11°¼; and ſal ammoniac, in the proportion of 1 : 17, ſinks it 7°; now 2¼ + 11¼ + 7 = 20¼; which, ſubtracted from 32, leaves 11°¾ for the computed freezing point of this mixture.

The moment I had found by experiment that the addition of a different ſalt to the ſaturated ſolution of any ſalt would ſtill further depreſs its freezing point, it was obvious to con-

clude, that greater cold could be produced with ſnow by a mixture of ſalts than by means of either taken ſeparately. I made ſeveral experiments with this view, and found it uniformly the fact, that by adding a certain proportion of a ſalt which had leſs power of producing cold with ſnow, to one which had a greater power, the frigorific effect of the latter was ſenſibly increaſed. Paſſing over examples of leſs conſequence, it will be ſufficient to inſtance common ſalt and ſal ammoniac. The ordinary common ſalt I uſed to mix with ſnow, ſunk the thermometer to $-5°$; the ſal ammoniac to $+4°$; but when ſome of the latter ſalt was mixed with the former, the compoſition produced with ſnow a cold of $-12°$. I did not carry the experiments far enough to determine the proportions which anſwered beſt, but ſaw evidently, that a conſiderable variety in this reſpect occaſioned ſcarcely a perceptible difference: more ſal ammoniac was, however, required, than that proportion of it to the common ſalt, which ought, by computation, to have ſunk a ſaturated ſolution of the latter 7 degrees. On ſeveral occaſions I made uſe of this compoſition to obtain a greater degree of cold than common ſalt alone would produce, and found it a very convenient method.

On this principle it is that impure common ſalt always makes a ſtronger freezing mixture than the pure; it being, in fact, a compoſition of ſalts. I have ſeen three ſalts produce a greater cold than two, but have not carried the experiments far enough to aſcertain the limits of this effect *.

M.

* The curious experiments, related in the Philoſophical Tranſactions for 1787, p. 282. of producing a great degree of cold by diſſolving a mixture of ſalts in water, depend in part upon the ſame principle, the water being capable of reducing more ſalt from a ſolid to a fluid ſtate when ſeveral kinds are employed,

M. DE REAUMUR propoſed to try what cold gunpowder would produce with ſnow, as a teſt of its goodneſs, that is, of the purity of the nitre in its compoſition. It is evident, from what has been delivered above, that if any ſaline impurity be mixed with the nitre, a frigorific mixture formed with the compoſition will be lower than $26°\frac{1}{2}$, a greater or leſs number of degrees, according to the nature and quantity of the impurity; and if it be only common ſalt, the proportion of it to the nitre may be thus determined pretty nearly.

As the cold produced by common ſalt with ſnow is $-4°$ or more, and by ſal ammoniac $+4°$, it is difficult to conceive in what manner FAHRENHEIT fixed the zero of his thermometer. All thoſe who have examined the few authentic paſſages to be found in authors upon this ſubject, will be ſenſible how vaguely they are expreſſed, leaving it doubtful whether he uſed common ſalt alone, ſal ammoniac alone, or both mixed together. There is no method of ſinking a thermometer exactly to $0°$ with theſe ſalts and ſnow, but by means of a certain ſmaller proportion of common ſalt added to the ſal ammoniac; and it would have been an extraordinary chance, that FAHRENHEIT ſhould hit preciſely this proportion often enough to make him rely upon the point ſo found as the commencement of his ſcale; more eſpecially as the proportion

employed, than it could of one of the kinds only. A remarkable fact of this nature, as I imagine, is recorded ſo early as in the Memoirs of the French Academy of Sciences for 1700, p. 116. "On prend une livre de ſublimé corroſif, et une "livre de ſel ammoniac; on les pulveriſe chacun à part; on mêle enſuite les "deux poudres très exactement; on met le melange dans un matras, verſant "par-deſſus trois chopines de vinaigre diſtillé; on agite bien le tout: et ce me- "lange ſe refroidit ſi fort, qu'on a peine à tenir long-tems le vaiſſeau dans les "mains en été. Il eſt arrivé même quelque fois à M. HOMBERG, que faiſant ce "melange en grande quantité, la matière s'eſt gelé."

is

is probably no greater than a ſeventh or a ſixth of common ſalt to the ſal ammoniac. Is it poſſible that FAHRENHEIT, finding a conſiderable difference in his experiments, took the mean between them for his zero, without any reſpect to the different nature of the ſalts with which he operated? It appears, that he was at this time ſo little acquainted with the ſubject, as to conſider his zero as the utmoſt limit of cold.

I come now to certain ſubſtances which, by equal additions, ſeem to depreſs the freezing point of water in an *increaſing ratio*. Theſe, as was mentioned before, are acids, alkalies, and ſpirit of wine. I judge this to be the caſe from the following experiments, though they are by no means ſo full and extenſive as I could have wiſhed to make them, had leiſure permitted.

The ſpecific gravity of the vitriolic acid I employed was 1,837 at 62° of heat; its effect upon the freezing point is ſhewn by the following table.

Vitriolic acid.

Proportion of water to acid.	Freezing point by the experiment.	Freezing point by calculation.
	°	°
10 : 1	24½	22½
5 : 1	12½	12⅘
4 : 1	7½	7½

This table is conſtructed in the ſame manner as thoſe formerly given of the ſolution of ſimple ſalts; the laſt experiment, where the proportion is 4 : 1, being taken as the ſtandard for computation; and the extreme difference between the calculation and experiment is no leſs than 2°½, upon a reduction

reduction of the freezing point from 24°½ to 7°½. The freezing point, set down in the table, is that to which the liquor rose upon congealing, after having been cooled several degrees lower; which it is proper to remark, because the ice rose two or three degrees in thawing.

The nitrous acid I employed was smoaking, and had its specific gravity 1,454. It acted on the freezing point according to the following table, which is constructed in all respects like the preceding.

Proportion of water to acid.	Freezing point by experiment.	Freezing point by calculation.
	°	°
16,8 : 1	26½	25⅔
10 : 1	22 –	21½
7,64 : 1	18	18
5,06 : 1	10½	11
4,26 : 1	7	7

The greatest difference between the calculation and experiment appears here to be only two-thirds of a degree; but that is more than, I think, can be attributed to inaccuracy. These mixtures cooled remarkably well; that in which the water was to the acid as 7,64 : 1 sunk down to 6° before it froze. The ice heated about a degree before it was melted.

Spirit of salt being a very weak acid, its increase of ratio was not perceptible within the limits to which I was confined.

Muriatic

Muriatic acid.

Proportion of water to acid.	Freezing point by experiment.	Freezing point by calculation.
	°	°
10 : 1	25	25
5,1 : 1	18$\frac{4}{5}$	18$\frac{4}{5}$
3,05 : 1	9$\frac{1}{2}$	9$\frac{1}{2}$

This table is conſtructed as the foregoing; and the ſpecific gravity of the marine acid was 1,163.

Salt of tartar, ſuch as is uſually ſold in the ſhops, was the vegetable alkali I employed. It did not readily deliqueſce, and conſequently was not very cauſtic: the cold it produced with ſnow was − 12°.

Salt of tartar.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
	°	°
10 : 1	27$\frac{1}{4}$	26$\frac{1}{4}$
7,5 : 1	25$\frac{1}{4}$	24$\frac{4}{5}$
5 : 1	22$\frac{3}{4}$	21$\frac{4}{5}$
3 : 1	15	14
2,5 : 1	11$\frac{1}{4}$	10$\frac{1}{2}$
2 : 1	5	5

Here the greateſt difference between the calculation and experiment is ſomething more than 1$\frac{1}{2}$ degree, in ſinking the freezing point from 22°$\frac{3}{4}$ to 5°; but in the higher freezing points of the table it is leſs, as well as in the lower. Perhaps

this irregularity in the experiments is in part to be aſcribed to an impurity in the ſalt of tartar; a turbid appearance, and at length a depoſition, took place in all theſe ſolutions, but principally in the ſtronger, occaſioned probably by tartar of vitriol, with which that ſalt is ſo frequently mixed.

The mineral alkali I tried, which was the cryſtallized ſoda of the ſhops, ſhewed no increaſe of ratio; but the ſcale of its operation was too ſmall for a proper judgement to be formed.

Mineral Alkali.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
	°	°
10 : 1	30 −	29 9/10
5 : 1	27 4/5	27 4/5

This ſalt would not remain ſuſpended in much greater proportion in the cooled water. I conſider the ſolutions as decreaſing in their freezing point by an equal ratio: poſſibly, if the ſalt of tartar had been cryſtallized, and perfectly ſaturated with fixed air, it would alſo have acted in the manner of a neutral ſalt, and produced no increaſe upon the ratio.

My volatile alkali, being the ſal volatilis ſalis ammoniaci, was tried only in two proportions.

Vola-

Volatile alkali.

Proportion of water to alkali.	Freezing point by experiment.	Freezing point by calculation.
	°	°
10 : 1	25	25¼
5,18 : 1	19	19

Here alſo there is no appearance of an increaſe of ratio, but rather the contrary.

The ſpecific gravity of the ſpirit of wine I employed was ,829 at 62°. It depreſſed the freezing point according to the following table.

Spirit of wine.

Proportion of water to ſpirit.	Freezing point by experiment.	Freezing point by calculation.
	°	°
8,5 : 1	24½	23¾
5 : 1	17	18
3,66 : 1	12½	12½
3 : 1	8½	8⅓
2,5 : 1	4	4

The total difference between the calculation and experiment, upon a reduction of the freezing point from 24°½ to 4°, is ¼ of a degree; but the intermediate points are very irregular, and in an oppoſite ſenſe, as if the ratio were decreaſing. It ſeems more probable, that ſome inaccuracy in the experiments, perhaps owing in part to the evaporation of the ſpirits, ſhould be

the occaſion of this, than that there ſhould be ſuch an irregularity in the law.

If any perſon ſhould alledge, that the difference between the obſerved and computed freezing points in the foregoing tables is not ſufficient to eſtabliſh the increaſe of ratio, I can only reply, that it appears to me greater than can reaſonably be aſcribed to error in the experiment; eſpecially as ſimilar experiments with neutral ſalts, not conducted more attentively, agreed ſo well together in pointing out a different law. It muſt be allowed, however, that the experiments do not ſhew any increaſe of ratio, except in the vitriolic and nitrous acids, ſalt of tartar, and, with more ambiguity, in ſpirit of wine: from analogy only I ſuſpect it to take place in the other acids, and in the mineral and volatile alkalies provided they are cauſtic. That a different law from what prevails in the neutral ſalts ſhould take place with theſe ſubſtances, ſeems not ſurpriſing, when it is conſidered how much ſtronger attraction they ſhew for water, and how much leſs limited the proportion is in which they will unite to it: for the ſame reaſons, I ſhould not think it extraordinary, if deliqueſcent ſalts, combined with water, ſhould be found to obſerve the ſame increaſing ratio in depreſſing the freezing point.

As other experiments upon the watery congelation of acids, and in lower degrees of cold, have been given to the public, it would have been deſirable to compare them with mine, and thus to have obtained a greater length of ſcale. But the difficulty of aſcertaining the preciſe ſtrength of different acids is ſo great, and a ſmall uncertainty in this reſpect may occaſion ſo much error, that I conceive little reliance can be placed, with regard to the preſent object, upon any experiments except ſuch as are

made by diluting the *ſame* acid with different proportions of water.

I ſhall conclude this Paper with the account of an experiment to determine the effect of ſalt upon the expanſion of water by cold. Pure water begins to ſhew this expanſion about the temperature of 40°, that is, 8 degrees above its freezing point. I put a ſolution of common ſalt, in the proportions of 4,8 parts of water to one of the ſalt, and conſequently whoſe freezing point was 8°$\frac{1}{3}$, into an apparatus I had uſed for other experiments of the ſame kind; and found that the ſolution continued to contract till it was cooled to 17°, but had ſenſibly expanded by the time it was cooled to 15°. Suppoſe the expanſion to have begun at 16°$\frac{1}{3}$, it would be juſt 8 degrees above its new freezing point. Hence we have reaſon to conclude, as far as one experiment goes, that the combination of a ſalt with water has no other effect upon its quality of expanding by cold, than to depreſs the point at which that quality begins to be ſenſible, juſt as much as it depreſſes the point of congelation.

It is probable, that the ſubject of this Paper will appear to ſome minute and trifling. Did it lead to nothing further, that opinion might be juſt. But it is by the accumulation of facts of this kind, and eſpecially ſuch as have been developed by Mr. CAVENDISH in his excellent Papers on the freezing of acids *, that the foundation muſt be laid for a general theory of the laws of coheſion, which may ultimately lead to a knowledge of the ſtructure upon which the intimate properties of

* Philoſophical Tranſactions, Vol. LXXVI. p. 241. and p. 166. of this Volume.

bodies

bodies depend; in the ſame manner as more accurate obſervations of the different motions of the great maſſes in the univerſe, paved the way to the ſublime diſcoveries of KEPLER and NEWTON.

XIX. *Additional Experiments and Observations relating to the Principle of Acidity, the Decompoſition of Water, and Phlogiſton.* By Joſeph Prieſtley, *LL.D. F. R. S. With Letters to him on the Subject, by Dr.* Withering, *and* James Keir, *Eſq.*

Read May 1, 1788.

WHEN I wrote the Paper, which the Society has done me the honour to order to be printed in their Tranſactions, I had found that the decompoſition of dephlogiſticated and inflammable air, by means of the electric ſpark, produced an *acid liquor*, which Dr. WITHERING found to be the *nitrous*; though I ſhould have obſerved, that he expreſſed ſome doubt whether the liquor did not alſo contain ſome other acid beſides the nitrous.

I have ſince that time been deſirous to aſcertain the *quantity of acid* producible from a given quantity of air; and with this view I gave Mr. KEIR as much of the liquor as I had collected from the decompoſition of about five hundred ounce meaſures of dephlogiſticated air, and the uſual proportion of inflammable air mixed with it. The liquor, he informed me, was 442 grains, of the ſpecific gravity of 1022 (that of water being 1000) and that it contained as much acid as was equivalent to 12.54 grains of concentrated acid of vitriol; which quantity of vitriolic acid is capable of ſaturating as much vegetable fixed alkali as is contained in 22½ grains of dry nitre,

or about 23¼ grains of nitre cryſtallized in mean temperature. The ſediment of the ſame liquor he alſo ſuppoſed to contain, at leaſt, as much acid as the liquor itſelf.

That this ſediment contains much acid, ſeems evident from this circumſtance, that, when it is firſt formed, it often emits ſmall bubbles, which riſe to the ſurface of the liquor, and continues to do ſo a conſiderable time. This was more particularly the caſe with the ſediment which I had from the tinned iron tubes. Theſe ſmall bubbles, I imagine, conſiſt of nitrous air (formed from the ſuperabundant acid vapour adhering to the metal and the water in the liquor) becauſe when a phial, half filled with this liquor, had ſtood about a week, the air on the ſurface of it inſtantly, and repeatedly, extinguiſhed a piece of lighted wood that was dipped into it.

From the preceding *data*, given me by Mr. KEIR (and making allowance for the indefinite quantity of water contained in the concentrated acid of vitriol) I am inclined to think, that not much more than one-twentieth part of dephlogiſticated air is the acidifying principle, and that nineteen parts are water.

This, I would however obſerve, relates to air fully ſaturated with water, in conſequence of its having been kept in jars ſtanding in water, ſo that I think it poſſible that the water in the drieſt dephlogiſticated air may not amount to more than nine-tenths of its weight. But I have not aſcertained, by any experiment, how much water any of the kinds of air are capable of holding in a diffuſed ſtate, without being any neceſſary part of their conſtitution.

Though Mr. KEIR found the greateſt part of the acid in the liquor with which I furniſhed him to be the *nitrous*, there were evident ſigns of its containing a ſmall portion of *marine* acid,

acid, by its making a precipitation with a ſolution of ſilver in nitrous acid. But this mixture of marine acid, he obſerves, is conſtantly found to accompany the production of nitre in the operations of nature. Whether the different ſubſtances from which the dephlogiſticated air was extracted made any difference in this caſe, I cannot tell; but that which I gave Dr. WITHERING was from minium, and that which Mr. KEIR examined was from manganeſe.

In the notes which I took of the firſt production of this liquor I termed it *blue*, and Dr. WITHERING alſo calls it blue, and once a *greeniſh blue*; but that which I gave Mr. KEIR, and all that I have got ſince, is a decided and deep *green*, which Mr. KEIR thinks to be owing to the phlogiſtication of the nitrous acid.

Thoſe philoſophers who are unwilling to admit the doctrine of phlogiſton, will perhaps ſay, that, in thoſe experiments in which a calx is revived by means of inflammable air, this air joins the dephlogiſticated air that was in the calx; and that, in conſequence of this, the metal, being freed from a foreign ſubſtance, reſumes its proper form and qualities, without having received any addition whatever.

But ſince it appeared in the preceding experiments, in which inflammable air was procured by means of ſteam, that the *metal* did not become a *calx*, except in conſequence of parting with inflammable air (or rather with ſomething which, when united to water, is inflammable air) it cannot be ſuppoſed to recover its metallic form without re-imbibing the ſame thing that it had loſt, which thing may be termed *phlogiſton*.

Conſequently, inflammable air being (in the opinion of thoſe who do not admit the doctrine of phlogiſton) the ſame

thing with the ſubſtance from which it was procured, only volatilized, and united to water, it will follow, that any ſubſtance imbibing inflammable air muſt become compounded with that other ſubſtance from which the inflammable air was produced; and therefore, all the inferences recited in the former Paper, which tend to eſtabliſh the doctrine of phlogiſton, muſt be admitted.

It will be aſked, what becomes of the dephlogiſticated air which is certainly expelled from red precipitate when it is heated in inflammable air, and converted into running mercury, if the inflammable air enters into the calx, in order to its becoming a metal? I anſwer, that it unites with a part of the inflammable air, and forms nitrous acid; for the water that is collected in this proceſs is ſtrongly acid, as appears by its turning the juice of turnſole red; but the quantity is ſo ſmall, that it will hardly be poſſible to aſcertain what acid it is. Analogy, however, decides clearly in favour of the nitrous.

It may be ſuppoſed, that in this experiment with *red precipitate*, the acid was that which had not been ſufficiently expelled from that ſubſtance in the proceſs by which it is made. But the reſult was the very ſame when I uſed *precipitate per ſe*; and on this occaſion I uſed a portion of that which I procured from M. CADET in Paris, of which mention is made in the ſecond Volume of my Experiments, p. 36.

On the other hand, that which is expelled from finery cinder, when it is heated in contact with inflammable air (and thereby becomes *iron*) is *pure water*, without any acid. This I found to be the caſe even when the inflammable air had been got from iron by oil of vitriol. Does not this prove that the

iron had imbibed what was properly *water*, and not any one conſtituent part of water only?

That water enters into the conſtitution of every kind of air I ſuppoſed, becauſe it certainly does into that of *inflammable*, *fixed*, and *dephlogiſticated* air, and becauſe none of them can be produced except by proceſſes in which water either certainly is, or may be well ſuppoſed to be, preſent. That *nitrous* air alſo contains water, I have lately found from the iron that is heated in it becoming a proper finery cinder.

At the publication of my laſt Volume of Experiments, I had found, that iron heated in nitrous air acquired weight, and that what remained of the air was phlogiſticated air. Having ſince that time repeated this experiment, and afterwards heated the iron, which was by this means increaſed in weight, in inflammable air, the iron loſt its additional weight, and water was copiouſly produced, as in the ſame proceſs with finery cinder, or, as I ſometimes call it, ſcale of iron.

As nitrous air may be deprived of its water, and become phlogiſticated air by heating iron in it, I find that it undergoes the ſame change by being repeatedly tranſmitted through hot porous earthen tubes, through which I ſome time ago diſcovered that vapour will paſs one way, while the air contiguous to the heated tube will paſs the other.

I firſt tried this proceſs with turnings of iron in the tube, by which means the iron was readily converted into finery cinder; but afterwards I found that the ſame change was produced in the nitrous air by the hot tube only. The two bladders which I made uſe of in this experiment (and by the alternate preſſure of which I made the air contained in them paſs through the hot tube) became *red*, juſt as any bladder does that is filled with nitrous air, and then expoſed to the influ-

ence of the atmoſphere till it becomes phlogiſticated air, as may be ſeen in my former experiments. In this manner I now always treat the bladders in which I make experiments on air. It prevents them from putrefying, and gives them a firmneſs of texture ſimilar to tanning.

That nitrous air contains water, and that this water can contribute to the formation of fixed air, is evident from the following experiment. I heated five grains of charcoal of copper in eight-ounce meaſures of nitrous air, till it was increaſed to ten-ounce meaſures, and the charcoal had loſt one grain. Examining the air, I found about one-fifth of it to be fixed air, and the remainder phlogiſticated. It ſeems, therefore, that nitrous air conſiſts of water, and ſomething that may be called the baſis of nitrous acid, or that ſubſtance which, when united to dephlogiſticated air, will make nitrous acid; and this ſeems to be pure phlogiſton, ſince it is found, as the preceding experiments ſhew, in the pureſt inflammable air. May we not hence infer, that the nitrous is the ſimpleſt of all the acids, and perhaps the baſis of all the reſt?

It is evident, that more water than enters into the compoſition of nitrous air is neceſſary for the change of it into what I have called *dephlogiſticated nitrous air*, becauſe the contact of iron will not, without water, produce that change in it.

Though fixed air, as I have ſhewn, contains water as well as nitrous air, it cannot be deprived of it, and be decompoſed, by the ſame means; for I have heated iron in it by a burning lens, and have alſo made it paſs repeatedly through a hot earthen tube containing turnings of iron, without producing any change in it.

In the former Paper I ſaid, that manganeſe of itſelf gives only the pureſt dephlogiſticated air by heat; but I have now a quantity

a quantity which, after giving pure air with a moderate heat, gives air that is more than half fixed air with a greater degree of heat. It is evident, as I have obſerved before, that there are very different kinds of manganeſe.

In theſe Papers I have ſuppoſed, with M. LAVOISIER and others, that the *principle of acidity* is in the dephlogiſticated air only; but as the acid is always formed by the union of this air and the inflammable, it may, perhaps, with equal probability be ſuppoſed to be in either of them, or to be a compound of them both.

Mr. WATT deſires me to mention it as his conjecture, that the nitrous acid is contained in the inflammable air as the acid of vitriol is in ſulphur, the phoſphoric in phoſphorus, &c.; and that the dephlogiſticated air does nothing more than develope the acid. Mr. KEIR, who was led to expect that an acid *muſt* be the reſult of the union of dephlogiſticated and inflammable air, becauſe ſome acid is always the conſequence of its union with other inflammable ſubſtances, thinks that both may be neceſſary ingredients in it. Farther experiments may throw more light on the ſubject.

TO THE REV. DR. PRIESTLEY.

DEAR SIR,

I at length ſubmit to your conſideration the trials which I made upon the liquors you produced, by the firing of *dephlogiſticated* and *inflammable air* in cloſe veſſels; and if you think

think proper you may tranſmit them to the Royal Society, by way of Appendix to your own Paper upon that Subject.

§ I.

Dec. 25, 1787. LIQUOR produced by burning, in a cloſe *copper* veſſel, *inflammable air* got from IRON by means of the ſteam of water, and *dephlogiſticated air*, expelled by heat only, from MINIUM.

This liquor was blue, and contained a reddiſh brown ſediment.

Exp. 1. *Terra ponderoſa lime water*,
2. *Common lime water*, and
3. *Cauſtic fixed alkali*, cauſed a greeniſh precipitation.
4. *Cauſtic volatile alkali* produced a beautiful deep blue precipitate, which was wholly re-diſſolved by the addition of more volatile alkali.
5. *Phlogiſticated alkali* gave a reddiſh brown precipitate.
6. *Nitre of lead*, no change.
7. *Nitre of ſilver*, a bluiſh precipitate.
8. *Litmus*, its colour firſt turned red, then curdled, and at length nearly deſtroyed.
9. The *filtering paper*, employed to ſeparate the ſediment, was a ſlow burning touch-paper.

§ II.

Dec. 30. LIQUOR produced, by burning in a cloſe *copper* veſſel, *inflammable air*, obtained as in § I. and *dephlogiſticated air*, expelled by heat only, from MANGANESE. This liquor was blue, and contained a reddiſh-brown ſediment.

Exp. 1—9. Similar to thoſe in § I.; and the reſults being in all points the ſame, I determined to mix the liquids together,

ther, the quantities being ſmall, and ſubmit them to further examination.

§ III.

Exp. 1. The united liquids being neutralized by the addition of *cauſtic vegetable alkali*, a beautiful green precipitate ſubſided.

2. This *precipitate* diſſolved entirely in cauſtic volatile alkali, changing to a beautiful blue.

3. The ſuper-natant liquid being ſlowly evaporated to dryneſs, the liquor that eſcaped during this proceſs was nothing but *pure water*.

4. The imperfectly cryſtallized reſiduum (3) effervesced upon the addition of concentrated vitriolic acid, and being ſubmitted to diſtillation, on the firſt application of heat, an orange-coloured vapour aroſe, which was condenſed in a receiver previouſly wetted with pure water.

5. The diſtilled liquor (4) had the ſmell and the taſte of *nitrous acid*, which inſtantly changed the blue of litmus, and that of violet paper to a red; and it acted upon ſilver.

6. *Terra ponderoſa lime water*, added to it, occaſioned no precipitation.

7. Saturated with vegetable alkali it converted cap paper into touch paper, and formed, upon a ſlow evaporation, cryſtals ſimilar to thoſe of nitre.

§ IV.

Jan. 21, 1788. Liquor produced by burning *inflammable air*, obtained as before, and *dephlogiſticated air* from *mercurius precip. ruber*, in a cloſe *tinned iron* veſſel.

This

This liquor was nearly colourleſs, but contained a large quantity of a light brown ſediment.

Exp. 1. 2. 3. 4. 5. 8. 9. Similar to thoſe of § 1. and 11. except the different colour of the precipitates, and that made by the phlogiſticated alkali being a fine blue.

10. The liquor ſaturated with vegetable alkali being ſlowly evaporated to dryneſs, and the precipitates mixed with the brown ſediment being put into a retort, ſome concentrated vitriolic acid was added, and heat was applied. A *very* faint ſmell of nitrous acid aroſe, but no viſible red vapour; nor did any thing diſtil into the receiver until the heat was very conſiderable.

11. The liquor then diſtilled was evidently acid; but this acid was wholly precipitated by terra ponderoſa lime water.

§ V.

Feb. 19. Having now received a larger quantity of the *liquor* and ſediment, produced as § 1. *viz.* 329 grains of the bluiſh green liquor, and 12 grains of the reddiſh brown ſediment, I proceeded to repeat the experiments, but the reſults correſponding with the former ones, I need only mention, that the liquor changed litmus red, and coagulated it; that with phlogiſticated alkali it gave a browniſh red, and with cauſtic vegetable alkali a blue precipitate; that after ſaturation with the vegetable alkali, both the dried ſalt and the ſediment being ſubmitted to diſtillation with the addition of concentrated vitriolic acid, an orange-coloured vapour inſtantly aroſe, and the peculiar ſmell of nitrous acid was ſtrongly perceived. The diſtilled liquor,

liquor, beſides having the taſte of nitrous acid, diſſolved ſilver; and when neutralized by vegetable alkali and ſlowly evaporated, afforded well-formed cryſtals of common nitre.

Theſe, Sir, are all the trials I have made with the liquors produced in your experiments. They pretty clearly prove the acid generated to be the ſame, whether the dephlogiſticated air was procured from red precipitate of mercury, from minium, or from manganeſe, and that this acid is the *nitrous acid*. It is not quite ſo clear why the liquor and ſediment in § 12. gave no ſtronger marks of the preſence of nitrous acid; but it is evident, that the acid had united itſelf to the iron, if not to the tin, of the veſſel employed: and I find, that when nitrous acid is fully ſaturated with iron by being boiled with it, and fixed alkali is added, this mixture ſubmitted to diſtillation, with the addition of concentrated vitriolic acid, yields no red vapours, and very little ſmell of nitrous acid.

I remain, &c.

W. WITHERING.

Birmingham,
February 22, 1788.

TO THE REV. DR. PRIESTLEY.

DEAR SIR,

I HAVE examined the green liquor which I received from you, with a view to determine the two points you wiſhed prin-

cipally to know, namely, the *quantity* and the *kind* of the contained acid.

The quantity of the green liquor was 442 grains, and its ſpecific gravity was to that of water as 1022 to 1000. When the green liquor was poured out of the phial, there remained at the bottom 72 grains of a brown powder, which upon examination was found to be a calx of copper, ſoluble in acids, but not in water.

The colour of the liquor ſhewed that it was a ſolution of copper. However, it gave no cupreous precipitation to poliſhed ſteel, till a few drops of acid had been added. This circumſtance ſeemed to ſhew the perfect ſaturation of the acid of the green liquor with the copper; and this complete ſaturation was further evinced by the very ſmall alteration of colour which litmus ſuffered on being mixed with this liquor. This teſt did indeed ſeem to acquire a little tendency to redneſs, but the effect was ſo minute as not to deſerve conſideration, and might probably be owing to ſome ſlight phlogiſtication, as it is called, of the acid contained, rather than to its ſuperabundance.

Upon evaporating the green liquor by expoſure to air, without the application of heat, no cryſtals were formed, but a dry green powder, inſoluble in water, but ſoluble in acids. You had before acquainted me with this fact. As ſolutions of copper in the nitrous acid are known to yield deliqueſcent cryſtals, and as no cryſtallization happened in this inſtance, although the acid was evidently nitrous, I was deſirous of diſcovering the cauſe of this difference between this liquor and common ſolutions of copper in the nitrous acid. Upon making ſome trials with this view, I found that there were three periods or ſtages to be diſtinguiſhed in the combination of copper with the nitrous acid. The firſt period is when the acid is ſuper-

abundant, and deliqueſcent cryſtals formed. The ſecond period is when the acid has been completely ſaturated, or perhaps ſuperſaturated, by repeated and alternate evaporation to dryneſs, and re-diſſolution in water, and then no cryſtals but a green powder is formed. The third period is when, by a further evaporation of acid and increaſe of heat, the green powder is changed into a brown or black calx. The complete ſaturation of the green liquor ſeems then to be the cauſe of the formation of the green powder on evaporation, inſtead of deliqueſcent cryſtals; and the production of the brown calx may be ſuppoſed to be either the reſult of a calcination of a part of the ſolution of copper which may have taken place in conſequence of the great heat excited by the inflammation of the two airs; or to have been formed by the action of the heated dephlogiſticated air on the copper.

In order to find the quantity of acid that was contained in the green liquor, I added, by ſlow degrees, to 100 grains of this liquor, a ſolution of mild fixed alkali, till no more precipitation took place, taking great care to ceaſe adding alkali as ſoon as the copper ceaſed to precipitate, to prevent a re-diſſolution of the precipitated metal. Upon examining how much of this alkaline ſolution was required to ſaturate a given quantity of oil of vitriol of the common ſpecific gravity, *viz.* to water as 1844 to 1000, I found that the 100 grains of green liquor required for its precipitation as much alkali as would ſaturate 2,837 grains of the above-mentioned acid of vitriol. And as the whole quantity of green liquor was 442 grains, its precipitation would require as much alkali as would be capable of ſaturating 12,54 grains of the concentrated vitriolic acid.

This quantity of vitriolic acid is capable of ſaturating as much vegetable fixed alkali, as is contained in 22½ grains of dry

nitre, or about 23¾ grains of nitre cryſtallized in mean temperature *.

The difficulty of finding even an approximation of the quantity of acid, or of air expended in forming the brown calx, muſt be very great; and this circumſtance ſhews the diſadvantage of uſing metallic veſſels, although in other reſpects more commodious for your large experiments than thoſe of glaſs. I think it is probable, that the quantity of acid expended in this manner is as much or more than the quantity retained in the ſolution; but I am ſorry I am not able to give you any grounds for a true determination. However, the experiment which I made with the calx was as follows. I diſſolved it in vitriolic acid, and having ſaturated with alkali the ſuperfluous acid, I precipitated the ſolution by a fixed alkali of known ſtrength, and I found that the quantity of alkali requiſite for this precipitation was as much as would ſaturate ten grains of the oil of vitriol before mentioned. This quantity of vitriolic acid is ſo much leſs than I ſhould expect, that I regret I have not an opportunity of repeating the experiment.

The next point to be determined is the *ſpecies* of acid that is contained in the green liquor. The acid produced in your former experiments of burning inflammable and dephlogiſticated airs was determined to be the nitrous; and every trial which I have made of this green liquor has convinced me, that the ſame acid is contained in it alſo. Upon adding to this liquor ſome vitriolic acid and diſtilled water, I obtained by

* I ſay the *mean temperature*, becauſe the proportion of water retained by ſalts varies very conſiderably, according to the temperature in which they are cryſtallized; the warmer the weather, the leſs water being retained. The conſideration of this fact ought to enter into the eſtimates of the proportions of the component parts of ſalts.

diſtillation a colourleſs acid liquor, which from the ſmell, the properties of diſſolving ſilver very readily, and of giving a deflagrating quality to paper ſoaked in this liquor when ſaturated with alkali, evidently manifeſted the preſence of the nitrous acid. But beſides the nitrous, which is undoubtedly the acid in greateſt abundance, I think I may venture to affirm the exiſtence alſo of the marine acid.

Upon adding to the green liquor a ſolution of ſilver in which the acid was ſuperabundant, the mixture became turbid; and when it was boiled, the minute particles collected and fell to the bottom in form of white flocks, which, on expoſure to light, became dark coloured, and were re-diſſolved by volatile alkali; all which appearances are indications of a luna cornea. Although theſe appearances left no doubt in my mind of the exiſtence of a marine acid, I am nevertheleſs much obliged to our ingenious philoſophical friend Mr. JOHNSON for reminding us, that Mr. CAVENDISH had alſo obſerved, in the acid produced in his experiment of paſſing the electric ſpark through a mixture of dephlogiſticated and phlogiſticated airs, a precipitation upon adding a ſolution of ſilver; from which, however, that moſt accurate philoſopher would not infer the preſence of marine acid, having diſcovered that the precipitation might be produced by the nitrous acid itſelf, when that acid is much phlogiſticated. The acid of the green liquor is in ſome degree phlogiſticated, as appears both from the ſmell which I perceived on adding the ſolution of ſilver, which, as I have mentioned, contained a ſuperabundant acid, and from the green colour of the liquor; for I have never been able to make any but blue or greeniſh-blue ſaturated ſolutions from copper and nitrous acid, although they ſometimes appear green from a ſuperabundant quantity of yellow acid, or from the yellow fumes

fumes during the ſolution. By adding a ſolution of melted nitre to the blue ſolutions of copper either in the nitrous or vitriolic acids, the phlogiſticated acid of the melted nitre immediately communicates a green colour. The marine acid tends more than the other acids to give a green colour; but I am inclined to impute the colour of your liquor principally to the phlogiſtication of the acid, becauſe upon adding a very ſmall portion of vitriolic, marine, or nitrous acid, the colour was immediately changed to a light blue. The ſolution of ſilver, being purpoſely acid, always produced this change of colour, and I therefore conſidered, that the phlogiſticated acid was expelled in the operation, and could not affect the precipitation.

Experiments have taught me, that an exceedingly ſlight degree of that quality which is called the phlogiſtication of nitre, is ſufficient to produce an alteration in the colour of ſolutions of copper, and that the colour will be changed when the degree of phlogiſtication is incapable of producing any precipitation of ſilver. Not only a ſolution of melted nitre will always change the colour of cupreous ſolutions, but even cryſtals of nitre, when mixed with the nitrous acid, and the mixture boiled to dryneſs upon copper, and re-diſſolved in water, will give a green ſolution. If cryſtals of nitre be diſſolved in a colourleſs ſolution of ſilver in nitrous acid, and this mixture added to a ſolution of copper in the ſame acid, the blue colour of the latter will be changed to a green, although neither the nitre nor ſolution of ſilver will ſeparately have this effect, neither will nitre diſſolved in a dilute nitrous acid. I do not know what degree of phlogiſtication may be required to precipitate ſilver, but I ſuppoſe it to be very great; at leaſt it is certain, that the degree of phlogiſtication capable of giving a green

green colour to ſolutions of copper is inſufficient to precipitate ſilver. Mr. CAVENDISH, when he relates the fact of the precipitation of ſilver, ſpeaks only of nitre, *much* phlogiſticated, poſſeſſing this property.

To obviate any ſuſpicion of the precipitation of ſilver being owing to the phlogiſtication of the acid, I added to ſome of the green liquor about an equal quantity of colourleſs nitrous acid and ſome diſtilled water, and I kept this mixture boiling half an hour, in order to diſengage and expel any phlogiſticated acid which it might contain. To the liquor, which remained perfectly pellucid, I added a ſolution of ſilver, upon which the precipitation immediately took place with all its uſual circumſtances, and in as great a degree as before.

If, upon examining the acids which you or others may hereafter obtain by the inflammation of airs, a mixture of marine acid be conſtantly found to accompany the production of the nitrous, the fact will be only analogous to all the other known productions of nitrous acid; in all which, either in the natural formation of nitre as in Spain and India, or in the nitre beds and walls made by art, a very large proportion of marine ſalts is conſtantly obſerved to accompany the nitre.

I ſhall conclude my letter with mentioning a fact, which I obſerved in making ſome experiments to find the cauſe of the formation of the green powder above-mentioned, and which, though not relative to the preſent ſubject, may be thought not unworthy of communication. It is the *decompoſition of common ſalt and ſeparation of its alkali by copper*. As I conceived the opinion that the formation of this green powder by evaporation of this green liquor was owing to the perfect ſaturation of the acid, among other modes of obtaining a perfectly ſaturated ſolution of copper, I ſprinkled upon plates of copper ſtrong ſolutions

of

of common ſalt and of ſal ammoniac. In a few days the ſal ammoniac was found to have converted the copper into a dry green calx; but the common ſalt acted more ſlowly upon the copper, and continued moiſt during ſeveral weeks. At length, however, at the end of two months, a dry green calx was formed, and I obſerved, that round the edges of the calx where it joined to the ſurface of the copper not acted upon, and where the liquor had laſt ſtood before it was dried, an efflorescence had formed of mild mineral alkali, which ſeems to have been ſeparated from its acid by the action of the copper on the acid, and perhaps aided by the affinity of the alkali itſelf to fixed air.

The ſal ammoniac alſo had probably been decompoſed by the copper, but the volatility of its alkaline baſis, when diſengaged, had prevented its appearance.

Wiſhing you may long continue to add to the numerous and ſplendid liſt of diſcoveries with which you have already enriched natural philoſophy,

I am, &c.

JAMES KEIR,

March 26, 1788.

XX. *On the Probabilities of Survivorſhips between Two Perſons of any given Ages, and the Method of determining the Values of Reverſions depending on thoſe Survivorſhips. By Mr.* William Morgan; *communicated by the Rev.* Richard Price, *D. D. F. R. S.*

Read May 8, 1788.

THE hypotheſis of an equal decrement of life, adopted by M. DE MOIVRE, for the purpoſe of facilitating the computations of life annuities, has not only been rendered unneceſſary by the late publication of many excellent tables deduced from real obſervations*, but has likewiſe been found ſo very incorrect in ſome caſes, that probably little or no recourſe will ever be had to it in future. But though the direct application of this hypotheſis may be laid aſide, there is danger of its not being entirely abandoned; and mathematicians may ſtill be led to reaſon from this principle, by deriving their rules from the *expectations* rather than from the *real probabilities* of life. The ingenious Mr. THOMAS SIMPSON has contented himſelf with this inaccurate method in his Select Exerciſes, and he has been followed in it by moſt other writers on the ſubject†. Even in thoſe caſes which involve only two lives, the errors are often

* See Dr. PRICE's Treatiſe on Reverſionary Payments, 4th edit.; and Mr. Baron MASERES on Life Annuities.

† Dr. PRICE's ſolutions of his 15th and 16th queſtions are exceptions to this remark.

conſiderable, eſpecially if the expectations are derived from the *London*, the *Sweden*, or any other tables, in which the decrements of life are unequal. But when three lives are involved in the queſtion, theſe errors are generally enormous; nor is it ever ſafe, when the ages of thoſe lives differ very much, to have recourſe to rules which are founded upon this principle. The three following problems, though the moſt common in the doctrine of ſurvivorſhips, have never hitherto been ſolved in a manner ſtrictly true. The ſecond of them is of particular importance, and I have taken much pains to examine how far Mr. SIMPSON's ſolution of it may be depended upon. It has, indeed, been ſolved by M. DE MOIVRE *, and Mr. DODSON †: but the firſt of theſe writers has erred moſt egregiouſly in the ſolution itſelf, and the other having derived his rule from a wrong hypotheſis, has rendered it of no uſe. It is much to be wiſhed that the ſolutions of all caſes in reverſions and ſurvivorſhips were deduced, like the three following ones, from the real probabilities of life. Moſt of thoſe which are now in uſe are at beſt but approximations, and can never be relied on with any tolerable degree of ſatisfaction.

PROBLEM I.

Suppoſing the ages of two perſons, A and B, to be given; to determine the probabilities of ſurvivorſhip between them from any table of obſervations.

SOLUTION.

Let a repreſent the number of perſons living in the table at the age of A the younger of the two lives. Let a', a'', a''',

* See Mr. DE MOIVRE's 17th problem, and Dr. PRICE's remarks upon it in his Treatiſe on Reverſionary Payments, Eſſay 3. Vol. I.

† See DODSON's Mathematical Repoſitory, Prob. 23. Vol. III.

a'''', &c. represent the decrements of life at the end of the 1st, 2d, 3d, 4th, &c. years from the age of A. Let b represent the number of persons living at the age of B the older of the two lives, and c, d, e, f, &c. the number of persons living at the end of the 1st, 2d, 3d, 4th, &c. years from the age of B. Supposing now it were required to determine the probability of B's surviving A in the first year. It is manifest that this event may take place either by A's dying before the end of the year and B's surviving that period, or by the extinction of both the lives, restrained however to the contingency of B's having died *last*. The probability that A dies in the first year, and that B survives it, is expressed by the fraction $\frac{a'c}{ab}$. The probability that both the lives die in this year is expressed by the fraction $\frac{a' \cdot \overline{b-c}}{ab}$; and as it is very nearly an equal chance that A dies *first*, this fraction should be reduced *one-half*, and then it will become $= \frac{a' \cdot \overline{b-c}}{2ab}$. Hence the whole probability of B's surviving A in the first year will be $= \frac{a'c}{ab} + \frac{a' \cdot \overline{b-c}}{2ab} = \frac{a' \cdot \overline{b+c}}{2ab}$. In the same manner the probability of B's surving A in the 2d, 3d, 4th, &c. years may be found $= \frac{a'' \cdot \overline{c+d}}{2ab} \ldots \frac{a''' \cdot \overline{d+e}}{2ab} \ldots \frac{a'''' \cdot \overline{e+f}}{2ab}$, &c. respectively; therefore, the whole probability of B's surviving A will be $= \frac{1}{ab} \times \frac{\overline{b+c}}{2} a' + \frac{\overline{c+d}}{2} a'' + \frac{\overline{d+e}}{2} a''' + \frac{\overline{e+f}}{2} a''''$, &c.

Having found by the preceding series the probability of B, the elder life's, surviving A the younger; the other expression,

which denotes the probability of A's ſurviving B, is well known to be the difference between the foregoing ſeries and *unity*.

The ſum of this ſeries might eaſily be determined from tables of the expectations of ſingle and joint lives *. But no ſuch table as the latter having ever been computed, it will by no means be found a laborious undertaking to compute a table of the probabilities of ſurvivorſhip between two perſons of all ages immediately from this ſeries, without having recourſe to the expectations of life. For if the probability of ſurvivorſhip between any two perſons be found, the probability between two perſons one year younger is obtained with little difficulty; and by proceeding in this manner a whole table may be formed in leſs time than would be neceſſary for computing one of the expectations of two joint lives. To exemplify what I have ſaid, I ſhall juſt ſet down a few operations for determining the probability of ſurvivorſhip, according to the Northampton Table of Obſervations †, between two perſons, whoſe common difference of age is 10 years.

* The above ſeries, expreſſing the probability of ſurvivorſhip, may be eaſily found, from the ſolution of the next problem, $= \frac{\beta \cdot \overline{K-AK} - c \cdot \overline{C-AC}}{2b}$; K denoting the expectation of one life one year younger than B; C the expectation of a life one year older than B; AK and AC the expectations of the joint lives of A and K, and of A and C, reſpectively; and β, as in that problem, the number of perſons living in the table of obſervations at the age of K.

† See Dr. PRICE's Treatiſe on Reverſionary Payments, Tab. 6. Vol. II.

Age

Age of B.	Age of A.	Probability of B's ſurviving A.	Probability of A's ſurviving B.
96	86	$\frac{1}{1\times145}\times\frac{0+1}{2}\times34=.1173$ . .	$1-.1173=.8887$
95	85	$\frac{1}{4\times186}\times\frac{4+1}{2}\times41+17=.1606$	$1-.1606=.8394$
94	84	$\frac{1}{9\times234}\times\frac{9\times4}{2}\times48+119.5=.2049$	$1-.2049=.7951$
93	83	$\frac{1}{16\times289}\times\frac{16+9}{2}\times55+431.5=.2420$	$1-.2420=.7580$
92	82	$\frac{1}{24\times346}\times\frac{24+16}{2}\times57+1119=.2720$	$1-.2720=.7280$
91	81	$\frac{1}{34\times406}\times\frac{34+24}{2}\times60+2259=.2897$	$1-.2897=.7103$
90	80	$\frac{1}{46\times469}\times\frac{46+34}{2}\times63+3999=.3022$	$1-.3022=.6978$

It may eaſily be ſeen from theſe ſpecimens in what manner the probabilities of ſurvivorſhip between two younger lives are deduced from the probabilities between two older lives, provided their common difference of age be the ſame; for the numbers 17 . . 119.5 . . . 431.5, &c. in the 2d, 3d, 4th, &c. ſeries are the ſums of the ſeries next preceding. Thus 17 is $=34\times\frac{1}{2}\ldots119.5$ is $=41\times\frac{1}{2}+17\ldots.431.5$ is $=48\times\frac{1.5}{2}+119.5$, &c. It may be neceſſary to obſerve further, that if the ages of the two perſons be equal, the probability of ſurvivorſhip between them being likewiſe equal, is expreſſed by the fraction $\frac{1}{4}$; and that this affords an inſtance of the accuracy of the foregoing inveſtigation; for the ſeries expreſſing the probability in this caſe is the ſame with this fraction, the chance of ſurvivorſhip becoming then (ſince $a=b$; $a'=b-c$; $a''=c-d$, &c.; and $\overline{b+c}\times a'=\overline{b+c}\times\overline{b-c}=b^2-c^2$, &c.) $=\frac{bb-cc}{2bb}+\frac{cc-dd}{2bb}$, &c. $=\frac{1}{4}$.

Mr.

Mr. SIMPSON, in his Treatiſe on Annuities and Reverſions * has given a curve whoſe area determines the probability of ſurvivorſhip between two perſons according to any table of obſervations. If one of the lives be not very young ſo that the equidiſtant ordinates may not be too few, this area is ſufficiently correct. But if the eldeſt of the two lives is under 20 years of age, it becomes neceſſary to aſſume ſo many equidiſtant ordinates to render the ſolution accurate, when the decrements of life are unequal, that the operation is rendered much too laborious for uſe; nor do I know that it can be neceſſary to have recourſe to this area in any caſe, eſpecially as the true probabilities of ſurvivorſhip are ſo eaſily computed from the preceding ſeries.

The following table has been formed in the manner deſcribed above; and as no ſuch table has ever been attempted before, I have been the more deſirous to render it complete, by computing the probabilities of ſurvivorſhip between two perſons of all ages, whoſe common difference is not leſs than ten years.

Inſtead alſo of ſuppoſing certainty to be denoted by unity, I have aſſumed 100 for this purpoſe; ſo that the ſums in the adjoining columns expreſs the number of chances in 100 which are in favour of B or A's ſurviving the other.

* Lemma 2. p. 100.

TABLE,

TABLE, shewing the probabilities of survivorship between two persons of all ages, whose common difference of age is not less than ten years, computed from the Northampton Table of Observations in Dr. PRICE's Treatise on Reversionary Payments.

Ten years difference.			Twenty years difference.			Thirty years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
11— 1	58.59	41.41	21— 1	52.46	47.54	31— 1	48.23	51.77
12— 2	51.36	48.64	22— 2	44 33	55.67	32— 2	39.36	60 64
13— 3	48.23	51.77	23— 3	40.88	59.12	33— 3	35.57	64.43
14— 4	45.98	54.02	24— 4	39.00	61.00	34— 4	32.86	67.14
15— 5	44.70	55.30	25— 5	37.69	62.31	35— 5	31.35	68.65
16— 6	43.43	56.57	26— 6	36.40	63.60	36— 6	29.85	70.15
17— 7	42.53	57.47	27— 7	35.48	64.52	37— 7	28.83	71.17
18— 8	41.91	58.09	28— 8	34.84	65.16	38— 8	27.98	72.02
19— 9	41.60	58.40	29— 9	34.52	65.48	39— 9	27.53	72.47
20—10	41.53	58.47	30—10	34.41	65.59	40—10	27.33	72.67
21—11	41.58	58.42	31—11	34.40	65.60	41—11	27.24	72.76
22—12	41.68	58.32	32—12	34.42	65.58	42—12	27.18	72.82
23—13	41.78	58.22	33—13	34.44	65.56	43—13	27.14	72.86
24—14	41.90	58.10	34—14	34.47	65.53	44—14	27.11	72.89
25—15	42.02	57.98	35—15	34.51	65.49	45—15	27.08	72.92
26—16	42.16	57.84	36—16	34.56	65.44	46—16	27.06	72.94
27—17	42.26	57.74	37—17	34.58	65.42	47—17	27.01	72.99
28—18	42.32	57.68	38—18	34.54	65.46	48—18	26.90	73.10
29—19	42.34	57.66	39—19	34 44	65.56	49—19	26.72	73.28
30—20	42.31	57.69	40—20	34.29	65.71	50—20	26.50	73 50
31—21	42.22	57.78	41—21	34.08	65.92	51—21	26.21	73.79
32—22	42.09	57.91	42—22	33.84	66.16	52—22	25.89	74.11
33—23	41.97	58 03	43—23	33.59	66.41	53—23	25.56	74 44
34—24	41.83	58.17	44—24	33.35	66.65	54—24	25.22	74.78
35—25	41.70	58.30	45—25	33.09	66.91	55—25	24.87	75.13
36—26	41.56	58.44	46—26	32.83	67.17	56—26	24.52	75.48
37—27	41.41	58.59	47—27	32.56	67.44	57—27	24.16	75.84
38—28	41.26	58.74	48—28	32.28	67.72	58—28	23.79	76.21
39—29	41.10	58.90	49—29	31.99	68.01	59—29	23.41	76.59
40—30	40.94	59.06	50—30	31.70	68.30	60—30	23.02	76.98
41—31	40.78	59.22	51—31	31.43	68.57	61—31	22.63	77.37
42—32	40.63	59.37	52—32	31.16	68.84	62—32	22.23	77.77
43—33	40.49	59.51	53—33	30.88	69.12	63—33	21.81	78.19

Ten

Ten years difference.			Twenty years difference.			Thirty years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
44—34	40.34	59.66	54—34	30.60	69.40	64—34	21.38	78.62
45—35	40.19	59.81	55—35	30.32	69.68	65—35	20.93	79 07
46—36	40.04	59.96	56—36	30.03	69.97	66—36	20.47	79.53
47—37	39.88	60.12	57—37	29.73	70.27	67—37	20.01	79.99
48—38	39.71	60.29	58—38	29.43	70.57	68—38	19.54	80.46
49—39	39.54	60.46	59—39	29.13	70.87	69—39	19.06	80.94
50—40	39.38	60.62	60—40	28.82	71.18	70—40	18.59	81.41
51—41	39.23	60.77	61—41	28.49	71.51	71—41	18.10	81.90
52—42	39.07	60.93	62—42	28.14	71.86	72—42	17.58	82.42
53—43	38.90	61.10	63—43	27.74	72.26	73—43	17.05	82.95
54—44	38.81	61.19	64—44	27.33	72.67	74—44	16.53	83.47
55—45	38.53	61.47	65—45	26.90	73.10	75—45	16.04	83.96
56—46	38.35	61.65	66—46	26.46	73 54	76—46	15.60	84.40
57—47	38.16	61.84	67—47	26.01	73.99	77—47	15.15	84.85
58—48	37.97	62.03	68—48	25.55	74.45	78—48	14.68	85.32
59—49	37.77	62.23	69—49	25.09	74 91	79—49	14.16	85.84
60—50	37.56	62.44	70—50	24.61	75.39	80—50	13.61	86.39
61—51	37.30	62.70	71—51	24.06	75.94	81—51	13.04	86 96
62—52	37.00	63.00	72—52	23.49	76.51	82—52	12.46	87.54
63—53	36.68	63.32	73—53	22.91	77.09	83—53	11.94	88.06
64—54	36.34	63.66	74—54	22.35	77.65	84—54	11.60	88.40
65—55	35.97	64.03	75—55	21.81	78.19	85—55	11.30	88.70
66—56	35.58	64.42	76—56	21.31	78.69	86—56	11.04	88.96
67—57	35.17	64.83	77—57	20.80	79.20	87—57	10.80	89.20
68—58	34.74	65.26	78—58	20.24	79.76	88—58	10.63	89.37
69—59	34.30	65.70	79—59	19.59	80.41	89—59	10.25	89.75
70—60	33.85	66.15	80—60	18.90	81.10	90—60	9.65	90.35
71—61	33.38	66.62	81—61	18.23	81.77	91—61	8.68	91.32
72—62	32.90	67.10	82—62	17.57	82 43	92—62	7.54	92.46
73—63	32.46	67.54	83—63	17.03	82.97	93—63	6.18	93.82
74—64	32.04	67.96	84—64	16.73	83.27	94—64	4.93	95.07
75—65	31.70	68 30	85—65	16.55	83.45	95—65	3.68	96.32
76—66	31.45	68.55	86—66	16.29	83.71	96—66	2.58	97.42
77—67	31 20	68.80	87—67	16.38	83.62			
78—68	30.90	69.10	88—68	16.44	83.56			
79—69	30.47	69.53	89—69	16.23	83.77			
80—70	30.00	70.00	90—70	15.67	84.33			
81—71	29 58	70.42	91—71	14.50	85.50			
82—72	29.19	70 81	92—72	13.05	86.95			
83—73	28 90	71.10	93—73	11 08	88.92			
84—74	29 02	70 98	94—74	9.24	90.76			
85—75	29.17	70.83	95—75	7.17	92.83			
86—76	29.25	70.75	96—76	5.12	94.88			

Ten

Ten years difference.		
Ages.	Probabilities.	
87—77	29.42	70.58
88—78	29 94	70.06
89—79	30.29	69.71
90—80	30.22	69.78
91—81	28.97	71.03
92—82	27.20	72.80
93—83	24.20	75.80
94—84	20.49	79.51
95—85	16.06	83.94
96—86	11.73	88.27

Forty years difference.			Fifty years difference.			Sixty years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
41— 1	43.57	56.43	51— 1	39.16	60.84	61— 1	34.94	65.06
42— 2	33.98	66.02	52— 2	28.87	71.13	62— 2	24.13	75.87
43— 3	29.85	70.15	53— 3	24.46	75.54	63— 3	19.52	80.48
44— 4	26.88	73.12	54— 4	21.28	78.72	64— 4	16.19	83.81
45— 5	25.20	74.80	55— 5	19.48	80.52	65— 5	14.28	85.72
46— 6	23.53	76.47	56— 6	17.68	82.32	66— 6	12.37	87.63
47— 7	22.30	77.70	57— 7	16.35	83.65	67— 7	10.92	89.08
48— 8	21.40	78.60	58— 8	15.35	84.65	68— 8	9.82	90.18
49— 9	20.86	79.14	59— 9	14.74	85.26	69— 9	9.12	90.88
50—10	20.59	79.41	60—10	14.42	85.58	70—10	8.73	91.27
51—11	20.45	79.55	61—11	14.22	85.78	71—11	8.46	91.54
52—12	20.35	79.65	62—12	14 06	85.94	72—12	8.25	91.75
53—13	20.26	79.74	63—13	13.90	86.10	73—13	8.05	91.95
54—14	20.18	79.82	64—14	13.76	86.24	74—14	7.88	92.12
55—15	20.11	79.89	65—15	13.62	86.38	75—15	7.75	92.25
56—16	20.05	79.95	66—16	13.50	86.50	76—16	7.68	92.32
57—17	19 96	80.04	67—17	13.35	86 65	77—17	7.59	92.41
58—18	19.81	80.19	68—18	13.14	86.86	78—18	7.43	92.57
59—19	19.59	80.41	69—19	12.86	87.14	79—19	7.19	92.81
60—20	19.31	80.69	70—20	12.53	87.47	80—20	6.90	93.10
61—21	18.96	81.04	71—21	12.11	87.89	81—21	6.55	93.45
62—22	18.55	81.45	72—22	11.65	88.35	82—22	6.16	93.84
63—23	18.12	81.88	73—23	11.20	88.80	83—23	5.81	94.19
64—24	17.68	82.32	74—24	10.75	89.25	84—24	5.56	94.44

Forty years difference.			Fifty years difference.			Sixty years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
65–25	17.22	82.78	75–25	10.32	89.68	85–25	5.32	94.68
66–26	16.76	83.24	76–26	9.91	90.09	86–26	5.11	94.89
67–27	16.29	83.71	77–27	9.50	90.50	87–27	4.90	95.10
68–28	15.81	84.19	78–28	9.07	90.93	88–28	4.73	95.27
69–29	15.32	84.68	79–29	8.61	91.39	89–29	4.48	95.52
70–30	14.83	85.17	80–30	8.15	91.85	90–30	4.13	95.87
71–31	14.34	85.66	81–31	7.70	92.30	91–31	3.63	96.37
72–32	13.84	86.16	82–32	7.27	92.73	92–32	3.10	96.90
73–33	13.35	86.65	83–33	6.88	93.12	93–33	2.48	97.52
74–34	12.87	87.13	84–34	6.60	93.40	94–34	1.94	98.06
75–35	12.42	87.58	85–35	6.35	93.65	95–35	1.40	98.60
76–36	11.99	88.01	86–36	6.12	93.88	96–36	0.95	99.05
77–37	11.56	88.44	87–37	5.91	94.09			
78–38	11.12	88.88	88–38	5.74	94.26			
79–39	10.64	89.36	89–39	5.48	94.52			
80–40	10.21	89.79	90–40	5.11	94.89			
81–41	9.68	90.32	91–41	4.55	95.45			
82–42	9.19	90.81	92–42	3.92	96.08			
83–43	8.76	91.24	93–43	3.15	96.85			
84–44	8.46	91.54	94–44	2.47	97.53			
85–45	8.18	91.82	95–45	1.80	98.20			
86–46	7.95	92.05	96–46	1.23	98.77			
87–47	7.74	92.26						
88–48	7.59	92.41						
89–49	7.32	92.68						
90–50	6.90	93.10						
91–51	6.17	93.83						
92–52	5.33	94.67						
93–53	4.32	95.68						
94–54	3.42	96.58						
95–55	2.51	97.49						
96–56	1.73	98.27						

Seventy

Seventy years difference.			Eighty years difference.			Ninety years difference.		
Ages.	Probabilities.		Ages.	Probabilities.		Ages.	Probabilities.	
71 — 1	30.49	69.51	81 — 1	24.95	75.05	91 — 1	18.93	81.07
72 — 2	19.45	80.55	82 — 2	14.37	85.63	92 — 2	9.18	90.82
73 — 3	14.87	85.13	83 — 3	10.34	89.66	93 — 3	5.53	94.46
74 — 4	11.59	88.41	84 — 4	7.63	92.37	94 — 4	3.12	96.88
75 — 5	9.80	90.20	85 — 5	6.34	93.66	95 — 5	2.12	97.88
76 — 6	7.98	92.02	86 — 6	4.92	95.08	96 — 6	1.15	98.85
77 — 7	6.60	93.40	87 — 7	3.85	96.15			
78 — 8	5.53	94.47	88 — 8	3.04	96.96			
79 — 9	4.84	95.16	89 — 9	2.50	97.50			
80 — 10	4.44	95.56	90 — 10	2.16	97.84			
81 — 11	4.18	95.82	91 — 11	1.86	98.14			
82 — 12	3.97	96.03	92 — 12	1.57	98.43			
83 — 13	3.80	96.20	93 — 13	1.25	98.75			
84 — 14	3.72	96.28	94 — 14	0.97	99.03			
85 — 15	3.68	96.32	95 — 15	0.70	99.30			
86 — 16	3.70	96.30	96 — 16	0.49	99.51			
87 — 17	3.73	96.27						
88 — 18	3.75	96.25						
89 — 19	3.67	96.33						
90 — 20	3.45	96.55						
91 — 21	3.09	96.91						
92 — 22	2.63	97.37						
93 — 23	2.10	97.90						
94 — 24	1.64	98.36						
95 — 25	1.18	98.82						
96 — 26	0.80	99.20						

PROBLEM II.

Suppoſing the ages of A and B to be given; to determine, from any table of obſervations, the preſent value of the ſum S payable on the contingency of one life's ſurviving the other.

SOLUTION.

Let r denote £ 1. increaſed by its intereſt for a year, and let all the other ſymbols be the ſame as in the preceding problem. Let the life of B alſo be ſuppoſed to be the older of the two lives; and then it will follow, by reaſoning as in the ſolution of that problem, that the preſent value of S to be received on death of A, ſhould that happen in the life-time of B, will be expreſſed by the ſeries $S \times \overline{\frac{b+c \cdot a'}{2abr} + \frac{c+d \cdot a''}{2abr^2} + \frac{d+e \cdot a'''}{2abr^3} + \frac{e+f \cdot a''''}{2abr^4}}$ &c. This ſeries may be reſolved into the two following: $\frac{S}{2} \times \overline{\frac{ca'}{abr} + \frac{da''}{abr^2} + \frac{ea'''}{abr^3} + \frac{fa''''}{abr^4}}$ &c. $+ \frac{S}{2} + \overline{\frac{ba'}{abr} + \frac{ca''}{abr^2} + \frac{da'''}{abr^3} + \frac{fa''''}{abr^4}}$ &c. The firſt of theſe two ſeries may be again reſolved into $\frac{S}{2} \times \overline{\frac{c}{br} - \frac{ca-ca'}{abr} + \frac{d}{br^2} - \frac{da-da'-da''}{abr^2} + \frac{e}{br^3} - \frac{ea-ea'-ea''-ea'''}{abr^3}}$ &c. $\left(-\frac{S}{2} \times \overline{\frac{da'}{abr^2} + \frac{ea'+ea''}{abr^3}}\right.$ &c. $\left.=\right) - S \times \overline{\frac{c}{2br} \times \frac{d}{cr} - \frac{da-da'}{acr} + \frac{e}{cr^2} - \frac{ea-ea'-ea''}{acr^2}}$ &c. Let B denote the value of an annuity on the life of B, C the value of an annuity on a life one year older than B, AB and AC the values of annuities on the joint lives of A and B and of A and C, and theſe ſeries will be $= \frac{S \times \overline{B-AB}}{2} - \frac{S \times c \times \overline{C-AC}}{2br}$. Again, the ſecond ſeries above mentioned, or $\frac{S}{2} \times \overline{\frac{ba'}{abr} + \frac{ca''}{abr^2} + \frac{da'''}{abr^3}}$ &c., by purſuing the ſame ſteps may be found $= \frac{\beta \times S}{2b} \times \overline{K-AK} - \frac{S \cdot \overline{B-AB}}{2r}$ where β denotes the number of perſons living at the age of a perſon one year younger than B, K the value of an annuity on that life, and

and AK the value of an annuity on the joint lives of A and K. The whole value of the ſurvivorſhip is therefore =

$$S \times \overline{\frac{r-1 \cdot B-AB}{2r} + \frac{\beta \cdot K-AK}{2b} - \frac{\xi \cdot A-AC}{2br}}. \quad \text{Q. E. D.}$$

Having now the value of the ſum S payable on the contingency of B's ſurviving A, the value of the ſame ſum, payable on the contingency of A's ſurviving B, is eaſily obtained by the well known method of ſubtracting the value found above from the whole value of the reverſion after the extinction of the joint lives of A and B. It is evident, that the exactneſs of the above rule muſt depend on the accuracy with which the values of the ſingle and joint lives are computed. Being poſſeſſed of ſuch tables for all ages, even with reſpect to the joint lives, I have computed the following values, in order that it may be ſeen how far Mr. SIMPSON's approximation * (the only rule now in uſe) may be depended on.

* It muſt be remembered, that the correction explained by Dr. PRICE, in Vol. I. p. 39, &c. of his Treatiſe on Reverſionary Payments, muſt be applied to Mr. SIMPSON's rule; that is, when the reverſion is a *ſum* and not an eſtate, the value found by the rule muſt be divided by £. 1, increaſed by its intereſt for a year.

Ago

Age of B.	Age of A.	* Value of £. 100 payable on the death of A if B survives him. True value.	Simpson's value.
10	2	32.67	26.05
10	10	24.74	24.75
20	2	29.99	24.73
20	10	22.11	23.50
20	20	27.95	27.96
30	2	28.79	22.60
30	10	19.84	21.47
30	30	30.22	30.21
40	2	26.65	20.07
40	10	17.10	19.07
40	40	32.86	32.87
50	2	23.36	17.06
50	10	14.01	16.21
50	20	18.65	19.29
50	50	35.80	35.85
60	2	21.52	13.61
60	10	10.65	12.93
60	30	17.51	18.19
60	60	38.28	38.92
70	2	18.36	9.81
70	10	7.07	9.15
70	40	15.35	15.78
70	70	42.34	42.33
80	2	14.46	5.71
80	10	3.93	5.43
80	50	12.05	12.00
80	80	45.47	45.45

From this table it appears, that Mr. SIMPSON's approximation in the middle ſtages of life is ſufficiently accurate; but that it is exceedingly defective when the life of A is very young. It ſhould alſo be remembered, that theſe values have been computed at a low rate of intereſt, and from the *Northampton* Table of Obſervations, in which the decrements of life come nearer to M. DE MOIVRE's hypotheſis than in any other table. But if the computations be made at a higher rate of intereſt even from this table, the approximation does not always agree ſo well, as will appear from the following ſpecimens calculated at 5 *per cent.*

* Theſe values have been computed at 3 *per cent.* and from the Northampton Table of Obſervations.

Age

Age of B	Age of A	Value of £. 100 payable on the death of A if B survives him, by the Northampton table. True value.	Simpson's value.
20	2	25.09	18.46
20	10	15.49	17.54
40	2	23.57	15.97
60	2	19.83	11.75
60	40	18.73	19.61*

Value of £. 100 payable on the death of A if B survives him, according to the Sweden Table of Observations, and at 4 *per cent.*

Age of B.	Age of A.	True value	Simpson's value.
20	2	21.47	16.80
20	14	15.42	17.82
20	20	20.01	19.84
40	4	23.53	14.22
40	16	13.71	16.23
40	28	17.60	20.44
40	40	27.62	27.00
60	24	9.39	13.01
60	36	12.29	16.81
60	42	16.11	19.58
60	60	36.96	36.34
76	40	9.21	9.81
76	52	12.58	14.00
76	64	23.81	22.81
76	76	42.90	43.29

In order further to compare Mr. Simpson's approximation with the true value, I have inserted in the foregoing table a few computations deduced from the *Sweden* Table of Observations, in which the decrements of life are unequal. From these instances the approximation appears to be more defective in proportion as the probabilities of life differ from the hypothesis. It may be proper, however, to observe, that this table has been computed from the values of two joint lives given by Dr. Price in his Treatise on Reversionary Payments, which values cannot be found with perfect correctness at every age, because given in that Treatise (Vol. II. p. 57, &c. and p. 144, &c.) only for ages that are either equal, or whose difference is five years or some multiple of five as in the *Northampton* Table, or six years or some multiple of six as in the *Sweden* Table. This circumstance may perhaps be supposed to affect the values of the survivorships computed from them, when the

* These values at 3 *per cent.* are 21.92 and 22.78.

joint lives differ from each other in a greater or leſs degree than they have been given in thoſe Tables. But by comparing the values of the reverſions when the ages are equal (in which caſe Mr. SIMPSON gives the true rule) it will be ſeen, that the values of the joint lives are ſufficiently correct for the purpoſe; and in order to put this matter out of all doubt, I have computed the values by the foregoing rule from the *exact* values of the joint lives by the *Northampton* Table, and alſo from thoſe values which have been deduced from Dr. PRICE's Tables, and I have found them to agree exceedingly well*.

It may be perceived, that in all the caſes of *equal* lives I have computed the values of the ſurvivorſhips by the preceding rule. This has only been done to prove its accuracy by comparing it with Mr. SIMPSON's rule, which is in this particular caſe, as I have already obſerved, perfectly right; for when the ages are equal, the chance of ſurvivorſhip muſt alſo be equal, and therefore *half* the value of the reverſion after the extinction of the joint lives will be the true value of the given ſum payable on the death of either A or B, ſubject to the contingency of his being ſurvived by the other life. The truth of this rule is ſelf-evident; nor does it at all ſeem to depend on Mr. SIMPSON's ſolution in his Select Exerciſes. That it is capable, however, of being deduced from the foregoing ſeries, may be demonſtrated in the following manner. By

* I ſhall juſt mention the following inſtances. When the age of B is 20 and that of A is 2 years, the value, by taking the joint lives from Dr. PRICE's Table, and approximating to the real values of thoſe lives in the manner directed by him, Vol. II. p. 75. of his Treatiſe, is 29.59. When the ages of B and A are 40 and 2, 50 and 20, 60 and 2, and 70 and 10, the reſpective values are 26.93, 18.78, 21.57, and 7.08, which being compared with the values in p. 344. will be found to agree almoſt exactly with them.

reaſoning

reasoning as in p. 335. the value of the sum S, when the ages are equal, will be $= S \times \frac{c}{br} \times \frac{b-c}{b} + \frac{\overline{b-c}^2}{2bbr} + \frac{d}{br^2} \times \frac{c-d}{b} + \frac{\overline{c-d}^2}{2bbr^2} +$ &c. $=$ $\frac{S}{2} \times \frac{bb}{bbr} + \frac{cc}{bbr^2} + \frac{dd}{bbr^3} +$ &c. $- \frac{S}{2} \times \frac{cc}{bbr} + \frac{dd}{bbr^2} + \frac{ee}{bbr^3} +$ &c. $= \frac{S}{2r} + \frac{S}{2r} - \frac{S}{2}$ $\times$ BB (putting BB for the value of the two equal joint lives) $=$ $\frac{S}{2} \times \frac{1}{r} - \frac{r-1}{r} \times$ BB $=$ (since the perpetuity P is $\frac{1}{r-1}$, and the interest of any sum is that sum multiplied by $r-1$) $\frac{S}{2} \times \frac{P.-1}{r} - \frac{BB.-1}{r}$, which is Mr. Simpson's rule with Dr. Price's correction; that is, "half the sum multiplied by the "difference between the perpetuity and the value of the equal "joint lives, and divided by £. 1 increased by its interest for a "year."

PROBLEM III.

The ages of A and B being given; to determine the value of the sum S, payable on the extinction of one life in particular, should that happen after the extinction of the other life.

SOLUTION.

Supposing B to be the oldest of the two lives and the sum S to become payable on his decease, it is evident that this payment at the end of the first year must depend on the contingency of both lives being extinct before this period and of B's dying *last*. Retaining the same symbols, and reasoning as I have done in the solution of the first problem, this value

will be expreſſed by the fraction $\frac{S \cdot d' \cdot \overline{b-c}}{2abr}$. The payment of the ſum S at the end of the ſecond year will depend on either of two events happening. Firſt, that A and B both die in the ſecond year after having ſurvived the firſt, reſtrained, as above, to the contingency of B's having died laſt; ſecondly, that B dies in the ſecond year and A in the firſt year. The value therefore of S for this year will be expreſſed by the two fractions $\frac{S \cdot a'' \cdot \overline{c-d}}{2ab^2} + \frac{S \cdot \overline{c-d} \cdot a'}{2b^2}$. Again, the payment of S in the third year will depend either on A and B's both dying in that year, and B having died *laſt*; or on B's dying in that year, and A's dying in the firſt or ſecond years. The value therefore of S for this year will be $= \frac{S \cdot d''' \cdot \overline{a-e}}{2abr^3} + \frac{S \cdot \overline{d-e} \cdot \overline{a'+a''}}{abr^3}$. By proceeding in this manner for the other years the whole value of the reverſion will be found $= \frac{S}{2} \times \overline{\frac{a' \cdot b-c}{abr} + \frac{a'' \cdot \overline{c-d}}{abr^2} + \frac{a''' \cdot \overline{d-e}}{abr^3} + \frac{a'''' \cdot \overline{e-f}}{abr^4}} + \&c. + S \times \overline{\frac{a' \cdot \overline{c-d}}{abr^2} + \frac{\overline{a'+a''} \cdot \overline{d-e}}{abr^3} + \frac{\overline{a'+a''+a'''} \cdot \overline{e-f}}{abr^4}} + \&c.$ The firſt of theſe ſeries by proceeding in the ſame manner as in the ſolution of the ſecond problem may be found $= \frac{R}{2b} \times \overline{K-AK} - \frac{\overline{B-AB}}{2r} - \frac{1}{2} \cdot \overline{B-AB} + \frac{c}{2br} \times \overline{C-AC}$; and the ſecond ſeries may be found $= -\frac{c}{br} \times \overline{C-AC} + \frac{\overline{B-AB}}{r}$. Hence the whole value of the reverſion will be $= S \times \overline{\frac{Br \cdot K-AK-c \cdot C-AC}{2br}} - \overline{\frac{r-1 \cdot B-AB}{2r}}$. Q. E. D.

Having now the value of the ſum S depending on the older of the two lives dying laſt, the value of the ſame ſum depending

depending on the younger of the two lives dying last is easily obtained, by subtracting the value first found from the whole value of the reversion after the extinction of both lives.

The answers computed by this rule differ rather more from those computed by Mr. SIMPSON's approximation than they do in the preceding problem. But I am fearful of becoming tedious, and therefore shall desist from inserting the comparative values in this case. It may not, however, be improper to exemplify the truth of this demonstration, by shewing in what manner the series may be resolved into the plain simple rule for computing the value of the reversion, when the ages are equal. The series in this case become

$$\frac{S\cdot\overline{b-a}^{2}}{2bbr}+\frac{S\cdot\overline{c-a}^{2}}{2bb_{1}^{2}},\ \&c.+\frac{S\cdot\overline{b-c}\cdot\overline{c-d}}{bbr^{2}}+\frac{S\cdot\overline{b-d}\cdot\overline{d-e}}{bb_{1}^{3}}+\&c.=$$

$$\frac{S}{2}\times\overline{\frac{bb}{bbr}+\frac{cc-2bc}{bbr^{2}}+\frac{dd-2bd}{bbr^{3}}+\&c.}+\frac{S}{2}\times\overline{\frac{2bc-cc}{bb_{1}^{2}}+\frac{2bd-dd}{bb_{1}^{3}}},\ \&c.=$$

$$S\times\overline{\frac{1}{2r}+\frac{1}{r}\times\frac{2B-BB}{2}-\frac{2B-BB}{2}}=\text{(putting L for 2B}-\text{BB, the}$$

value of an annuity on the longest of the two equal lives)

$$\frac{S}{2}\times\overline{\frac{1}{r}+\frac{1-r\cdot L}{r}}=\frac{S}{2}\times\frac{P\cdot\overline{r+1}}{r}-\frac{L\cdot\overline{r-1}}{r}.\quad \text{Q. E. D}^{*}.$$

The *exact* value of all reversions depending on survivorships between two lives might be found in the same manner as the values in the preceding problems. With regard to the values of reversions depending on survivorships between three lives, I am sensible that the solutions of those cases would be rather difficult when deduced from the real probabilities of life. But they certainly might be effected; and *these* are the more necessary, inasmuch as the solutions derived from the expectations of life are often so very defective as not to deserve the name of approximations.

Chatham Place, Feb. 2, 1788.

* See the latter part of the solution of the second problem, p. 347.

XXI. *An Account of a remarkable Tranſpoſition of the Viſcera.* By Matthew Baillie, *M. D.* *In a Letter to* John Hunter, *Eſq. F. R. S.*

Read May 8, 1788.

TO JOHN HUNTER, ESQ. F.R.S.

DEAR SIR,

A VERY ſingular variety having occurred lately in the ſtructure of the human body, I beg leave to communicate an account of it by your means to the Royal Society, if you ſhould think it worthy of their notice. It happens by a very uncommon concurrence of circumſtances, that while I am naturally led by the ties of affinity to apply to you upon this occaſion, I can gratify my pride by thinking it is at the ſame time to the perſon who is poſſeſſed of the firſt reputation for his unwearied reſearches in one of the moſt extenſive, as well as intereſting, parts of the ſyſtem of nature.

I am, &c.

M. BAILLIE.

Great Windmill-ſtreet,
April 12, 1788.

THERE

THERE is nothing which tends more to illuſtrate the powers and the wiſdom of nature than the inveſtigation of the ſtructure of animals. We there find a moſt wonderful delicacy of mechaniſm, and exquiſitely adapted to a variety of purpoſes. This, however, is not to be better ſeen by following nature in her common tract than by obſerving her wanderings. In theſe ſhe often ſhews more particularly the extent of her powers, and throws light on her ordinary plans. It is ſuch circumſtances which give importance or value to the obſervation of ſingular phænomena. The variety in animal ſtructure, an account of which I have the honour of preſenting to this learned Society, is a complete tranſpoſition in the human ſubject of the thoracic and abdominal viſcera to the oppoſite ſide from what is natural.

It is ſo extraordinay as ſcarcely to have been ſeen by any of the moſt celebrated anatomiſts, and indeed has been but very generally noticed at all. The circumſtance has been mentioned, but it has not been particularly deſcribed ſo as to make it thoroughly known, or to eſtabliſh its certainty. It was hanging in the minds of many as doubtful, whether ſuch a variety did really exiſt. There is one circumſtance that attends the account of the preſent caſe, which has not always happened in the record of ſingular phænomena, *viz.* that it has been examined by phyſicians and ſurgeons of the firſt reputation in this large town, and has been in ſome meaſure open to the gratification of public curioſity. Such a circumſtance (were it neceſſary) would give the ſtamp of authenticity to appearances even much farther removed from the ordinary plans of nature.

I have

I have been at the pains to consult many authors upon this subject, but with very little satisfaction. I shall not enter into a detail of what I have met with in the course of these researches, but shall briefly notice, that when any lusus of this sort is mentioned, it is commonly in a single sentence or two, and the transposition is not marked as universal, or it is a change in the situation of some viscus from disease. In short, I have only found this singular lusus naturæ described by CATTIERIUS, M. MERY, and M. DAUBENTON; but by none of them is it sufficiently particular. Enough has been said to point out that they had exactly met with the same sort of monstrosity; but many circumstances have been omitted, which I hope will be supplied by the present account, which I proceed immediately to lay before the Society.

The person who is the subject of this Paper was a male, near forty years of age, somewhat above the middle stature, and of a clean active shape. He was brought for dissection in the common way to Windmill-street. Upon opening the cavity of the thorax and abdomen, the different situation of the viscera was so striking as immediately to excite the attention of the pupils who were engaged in dissecting it; and Mr. CRUIKSHANK, as well as myself, were very soon informed of the singularity. We were much surprized as well as pleased with the appearance, and I began immediately to examine every part of the change with considerable attention: for this purpose, after desiring a drawing to be made of the appearances as they were found upon opening the body, I next day injected it. The repeated dissections have furnished various views, which are represented faithfully by drawings, and which I hope will enable me to give a tolerably distinct account of this singular lusus naturæ. I shall not enter in my description into unnecessary minutiæ: this would render the

Paper leſs ſuited to the Society, would not convey more information to perſons thoroughly acquainted with anatomy, and would rather tend to obſcure what is more important to thoſe who have not given ſo much attention to ſubjects of this nature. It may not be improper to obſerve, that, beſides the tranſpoſition in the viſcera of this perſon, there are ſeveral peculiarities which ſometimes occur. I have taken notice of them in my deſcription, although they are entirely independent of the tranſpoſition.

Deſcription of the thorax.

The mediaſtinum, or anterior duplicature of the pleura ſeparating the two cavities of the cheſt from each other, was found to incline obliquely downwards to the right ſide fully as much as it does commonly to the left ſide of the cheſt. The pericardium too inclined obliquely to the right ſide. On preſſing it gently away from the lungs the phrenic nerves came diſtinctly into view, in their common ſituation; but the right phrenic nerve ran more obliquely, and was longer than the left. The lung upon the right-ſide was divided by a ſingle oblique fiſſure into two lobes, having at the ſame time a deficiency oppoſite to the apex of the heart; and the lung on the left-ſide was divided into three lobes, exactly contrary to what is found in ordinary caſes.

On opening the pericardium the apex of the heart was found to point to the right-ſide nearly oppoſite to the ſixth rib, and its cavities as well as large veſſels were completely tranſpoſed. What are commonly called the right auricle and ventricle were ſituated on the left-ſide, and the left auricle and ventricle on the

the right. The pulmonary artery aſcended towards the right-ſide of the cheſt. The aorta was alſo directing its arch to the right; and the vena cava ſuperior, as well as inferior, were ſeen opening into their auricle on the left-ſide of the ſpine. There was nothing remarkable in the ſize or general figure of the heart.

On the outſide of the pericardium the tranſpoſition of the larger veſſels was very ſtriking. The longer ſub-clavian vein was paſſing from the left-ſide obliquely to the right before the branches which are ſent off from the arch of the aorta. The left carotid and ſub-clavian arteries were found to ariſe from the arch of the aorta by one common trunk; the right carotid and ſub-clavian ſeparately.

In the duplicature of the pleura behind, or what may be called the poſterior mediaſtinum, there was a change correſponding to what we have already deſcribed. The deſcending aorta was found paſſing on the right ſide of the ſpine. The œſophagus was before it, inclining more and more to the right towards its lower extremity, and it at length perforated the diaphragm ſomewhat on the right-ſide of the ſpine *. The thoracic duct was ſeen in the middle between the deſcending aorta and the vena azygos in ſome places forming a plexus of ſmall branches, in another dividing itſelf into two branches, which afterwards re-united in a common trunk, and at length climbing up to terminate in the angle between the jugular and ſub-clavian veins on the right-ſide of the body. The recurrent nerve of the parvagum on the right-ſide paſſed round the beginning of the deſcending aorta, and on the left paſſed round

* The vena azygos was on the left-ſide of the ſpine opening in the common way into the vena cava ſuperior, which we formerly mentioned to be alſo tranſpoſed in its ſituation.

the

the common trunk of the carotid and ſub-clavian arteries. The large intercoſtal nerves being exactly under the ſame circumſtances on each ſide, it was impoſſible there could be any tranſpoſition in them. It appears then, from the foregoing deſcription, that every thing admitting of ſuch a change was completely tranſpoſed in the thorax.

Of the abdomen.

The liver was ſituated in the left hypochondriac region, the ſmall lobe being towards the right, and the great lobe in the left-ſide. The ligaments uniting it to the diaphragm correſponded to this change, the right tranſverſe ligament being longer, and the left being ſhorter, than uſual. The ſuſpenſory ligament could undergo little change, except being puſhed to the left-ſide along with the liver. On preſſing upwards the liver, ſo as to exhibit its poſterior and under ſurface, the gall bladder was ſeen on the left-ſide preſerving its proper relative ſituation to the great lobe of the liver, and the veſſels of the portæ were found upon diſſection to be tranſpoſed correſponding to the change of circumſtances. The hepatic artery was found climbing up obliquely from the right towards the left, before the lobulus ſpigelii, and entered at the portæ into the ſubſtance of the liver by two or three branches on the right of the other veſſels. The ductus communis cholidochus was on the left of the other veſſels, being formed from the ductus hepaticus and ductus cyſticus in the common way, and it paſſed obliquely downwards on the left, to terminate in the duodenum. What was moſt remarkable, of which indeed I never ſaw or heard of any inſtance before, it terminated in the fore-part of the duodenum. The vena portarum paſſed behind the hepatic

artery and ductus communis cholidochus, aſcending obliquely towards the left-ſide.

The ſpleen was ſituated in the right hypochondriac region, adhering to the diaphragm in the common way. What was very remarkable was, there being three ſpleens, nearly of the ſize of a pullet's egg, found adhering to the larger ſpleen by ſhort adheſions, beſides two other ſtill ſmaller ſpleens which were involved in the epiploon at the great end of the ſtomach. I never ſaw ſo many ſmall ſpleens in any one ſubject. The pancreas was found on the right-ſide behind the ſtomach, running obliquely from the ſpleen to the curvature of the duodenum, and had its duct entering in common with the ductus communis cholidochus into the cavity of that inteſtine. The ſplenic veſſels were paſſing along the upper edge of the pancreas to the right-ſide, correſponding to the change of ſituation in the pancreas and ſpleen.

The ſtomach was ſituated on the right-ſide, partly hid by the ſmall lobe of the liver paſſing to the left, and terminating in the pylorus, rather on the left ſide of the ſpine. The duodenum took a moſt ſingular courſe; it firſt paſſed to the right-ſide, behind the ſmall end of the ſtomach; it then turned upon itſelf, towards the left-ſide; it afterwards took its proper ſweep to the right-ſide, paſſing behind the ſuperior meſenteric artery and meſaraica major vein. The meſentery began to be formed on the right-ſide, inſtead of the left, as in ordinary caſes. The ilium terminated in the great inteſtine on the left-ſide, and there was in it a diverticulum of conſiderable ſize, a luſus not unfrequently occurring. The cæcum was ſituated on the left pſoas magnus and iliacus internus muſcles. The tranſverſe arch of the colon paſſed from the left to the right-

ſide of the body, and the ſigmoid flexure croſſed over the right pſoas, to get into the cavity of the pelvis.

The kidnies had their veſſels tranſpoſed, as we ſhall remark more particularly afterwards; the renal capſules had undergone no change, as no variety could be produced by a tranſpoſition.

The aorta paſſed between the crura of the diaphragm into the cavity of the abdomen, and adhered in its courſe to the ſpine on the right-ſide of the vena cava inferior. Its branches were directed in their courſe correſponding to the peculiar ſituation of the viſcera. The ſplenic and coronary arteries were paſſing to the right-ſide, and the hepatic artery obliquely to the left. The ſuperior and inferior meſenteric arteries were directed to the right-ſide. There was no change in the ſpermatic arteries, any tranſpoſition in the teſticles (if ſuch a thing could take place) not being capable of affecting them. The lumbar arteries could alſo undergo little change, except that the left lumbar arteries muſt neceſſarily, from the peculiar ſituation of the aorta, be the longeſt. The vena cava inferior perforated the tendinous portion of the diaphragm, and adhered in its courſe to the ſpine on the left-ſide of the aorta.

The right emulgent vein was much longer than uſual, paſſing from the right kidney before the aorta to terminate in the vena cava ſuperior; and the left emulgent much ſhorter, paſſing from the left kidney to the vena cava, which was ſituated on the left ſide of the ſpine. The right ſpermatic vein was found to open into the right emulgent, and the left into the vena cava inferior, about an inch under the left emulgent. The vena portarum was changed from its natural courſe, paſſing obliquely upwards to the left-ſide, and its large branches, *viz.* the vena ſplenica, meſaraica major and minor, were all directed towards the right-ſide of the ſpine.

There was no change in the intercostal nerve within the cavity of the abdomen; nor does it seem to be capable of being affected by any transposition of parts. We see then, that there was a complete transposition of the abdominal viscera, each of them preserving its proper relative situation to the others.

I examined the brain, organs of sense, of generation, the muscles and blood vessels of the extremities, but found nothing in them remarkable. Indeed, I had no expectation of it; for all these parts are perfectly independent of thoracic or abdominal viscera; but I did it to satisfy myself and the curiosity of others, who might wish to put such a question, or have such a question arising in their minds.

The person seems to have used his right-hand in preference to his left, as is usually the case, which was readily discovered by the greater bulk and hardness of that hand, as well as the greater fleshiness of the arm. It was not to be expected he should be left-handed; but I mention this circumstance too, with a view to satisfy a curiosity which I know has been excited in many who have heard of this lusus.

I have been at considerable pains to learn something of the history of this person during life; but the particulars I have heard are applicable only to the circumstances of common men, having no connection with singularity of structure, and therefore, I think, it would be abusing the time of the Society to give any account of them. One thing it may be right to mention is, that the person, while alive, was not conscious of any uncommon situation of his heart; and that his brother, whom I have seen, has his heart pointing to the left-side as in ordinary cases. Indeed, there was little reason to expect that we should meet with any thing particular in the account of his

life. His health could not be affected by ſuch a change of ſituation in his viſcera; nor could there ariſe from it any peculiar ſymptoms of diſeaſe. Still leſs could there be any connection between ſuch a change and his diſpoſitions, or external actions. He might have known that his heart was directed towards the right-ſide; but if we conſider how little every perſon, eſpecially thoſe of the lower claſs, are attentive to circumſtances not very palpable, it was ſcarcely to be expected he ſhould know of it. If I had met with any thing in his life which was at all referable to the ſingularity of ſtructure, I ſhould have been very glad to have gratified the public curioſity by giving an account of it *.

Every ſingular phænomenon in animal ſtructure is worth remarking, even if it ſhould not lead immediately to any uſeful obſervation; but it becomes more important if it ſhould tend to throw any light upon the principles of nature in the formation of animals. It is reaſonable to think, that nature ſhould follow ſome general plan in her operations. There is ſome effect which ſhe has in view, and ſhe will generally employ the ſame means to produce it. In the ſtructure of any animal, her view is to form ſuch a combination of parts as to render the animal fitted for certain purpoſes. She will commonly form the ſame combination where the ſame purpoſes are to be ſerved; or, in other words, there will be the ſame ſtructure in the ſame ſpecies of animals. The ſame effect however may be produced, without a ſtrict adherence to the employment of the ſame means, as we find to be the caſe

* Since the above luſus has occurred, I have ſeen, in the poſſeſſion of Mr. PAYNE, Surgeon, a fœtus at the full time, with the viſcera tranſpoſed. In the Anatomical Collection of Chriſt Church, in Oxford, there is a heart tranſpoſed that had belonged to a very ſmall fœtus; but the fœtus itſelf is not preſerved.

in

in all human inventions, and therefore there is no reaſon why nature ſhould not ſometimes deviate from her ordinary plans. Accordingly we find there is much variety in animal ſtructure; but this does not commonly affect the animal functions. Under this reſtriction the variety is ſo great in the appearances of every part of an animal, that it is almoſt impoſſible to examine any two animals of the ſame ſpecies without remarking many differences.

In the bony compages of an animal we find little variety in the extremities of bones where there is the apparatus of a joint, becauſe a particular ſhape is beſt adapted to a particular kind or latitude of motion. In other parts of the bones, where a difference of features is not material, there is great variety, as in the foramina, depreſſions, ridges, and ſutures of bones.

The ſame general rule will apply to variety in muſcles. The principal object is a certain inſertion near a joint, ſo as to give a determined direction of motion. With reſpect to ſuch inſertions, there is, comparatively ſpeaking, little variety; but there is a great difference in the bodies and connections of muſcles, which have no ſhare in the regulation of the motion.

There is no part of an animal where there is a greater latitude of variety than in the diſtribution of blood veſſels. The reaſon of it is very obvious. The only object in the diſtribution of blood veſſels is, to carry blood to every part of the body and bring it back to the heart. The parts of an animal, in order to be ſupported, muſt be viſited by ſucceſſive changes of freſh blood; but it ſurely cannot be an object of importance whether the blood paſſes by one rout or another. Hence the variety in blood veſſels is extremely great. Still, however, there is a method in the deviations of nature, ſo that they

may

may be marked or noted, the ſame varieties occurring in different animals.

It cannot be at all important to the function of a viſcus, whether it be in one maſs, or in ſeparate portions. The ſtructure being the ſame, the ſame action will take place. Hence we often find the two kidnies joined together, forming one maſs; and not unfrequently two or three ſpleens beſides the common one. Neither can it be important, whether a viſcus ſhould always be of the ſame ſhape, becauſe its functions do not depend on ſhape, but on ſtructure: we find accordingly, in this particular, much variety.

There are many of the viſcera which are connected together in their functions, or by the junction of large blood-veſſels, in ſuch a way as to require nearly the ſame relative ſituation among themſelves. This becomes alſo neceſſary in order to preſerve the general ſhape of the animal. Accordingly we find, that when any important viſcus is changed in its ſituation, it affects the ſituation of other viſcera, requiring in them a ſimilar change. We ſaw in the perſon who is the ſubject of this Paper, that a change in the ſituation of the heart and liver was accompanied with a change of ſituation in the ſtomach, ſpleen, pancreas, and in ſhort the whole abdominal viſcera. This, however, is a great deviation in nature; for it is nothing leſs than changing almoſt the whole vital ſyſtem in an animal, and therefore it rarely happens.

In ſuch a change it does not appear, that the functions can be affected, as they depend on ſtructure and ſituation, which are both preſerved. Hence the perſon who is the ſubject of this Paper arrived at the age of maturity, and might have continued to live to an extreme old age.

The

The human machine might have been conſtructed in this way generally, and under ſuch circumſtances what is now called the natural ſituation of parts would have been as ſingular as the preſent phænomenon.

There appears to be leſs variety in the nervous ſyſtem of animals of the ſame ſpecies than in moſt parts of the body. There is ſcarcely any difference in the appearance of the brain, and much leſs in the diſtribution of the nerves than of the blood-veſſels.

There is alſo little variety in the organs of ſenſe: perhaps the mechaniſm in both theſe is nicer, ſo that a conſiderable deviation would interfere with their peculiar functions.

The moſt common great deviations which nature produces in the ſtructure of an animal are various kinds of monſtroſity, by which the animal becomes often unfit for continuing its exiſtence. Why nature ſhould in its greater deviations fall into a very imperfect formation, much below the ſtandard of her common work, does not appear very obvious.

It ſeems that there might have been many varieties where the functions could have been preſerved. Perhaps it is with a view to check the propagation of great varieties, ſo as to preſerve an uniformity in the ſame ſpecies of animals.

It has been much agitated, whether monſtroſities depend on the original formation, or are produced afterwards in the gradual evolution of an animal.

This does not appear to be a queſtion of much importance; nor perhaps can it be abſolutely determined. But upon the whole it is more reaſonable to think, that the ſame plan of formation is continued from the beginning, than that at any ſubſequent period there is a change in that plan.

It

It may be obſerved, that it is exactly the ſame creative action which produces the natural ſtructure, or any deviation from it; for in caſes of deviation the action is either carried too far, ceaſes too ſoon, or is diverted into uncommon channels. This will explain the various kinds of monſtroſity from redundancy, deficiency, or tranſpoſition of parts.

XXII. *On the* Georgian *Planet and its Satellites.*

By William Herſchel, *LL.D. F. R. S.*

Read May 22, 1788.

IN a Paper, containing an account of the diſcovery of two ſatellites revolving round the Georgian planet, I have given the periodical times of theſe ſatellites in a general way, and added that their orbits made a conſiderable angle with the ecliptic. It is hardly neceſſary to mention, that it requires a much longer ſeries of obſervations, to ſettle the mean motions of ſecondary planets with accuracy, than I can hitherto have had an opportunity of making; but ſince it will be ſome ſatisfaction to aſtronomers to be acquainted with ſeveral of the moſt intereſting particulars, as far as they can as yet be aſcertained, I ſhall communicate the reſult of my paſt obſervations; and believe that, conſidering the difficulty of meaſuring objects which require the utmoſt attention even to be at all perceived, the elements here delivered will be found to be full as accurate as we can at this time expect to have them ſettled.

The moſt convenient way of determining the revolution of a ſatellite round its primary planet, which is that of obſerving its eclipſes, cannot now be uſed with the Georgian ſatellites, as will be ſhewn when I come to give the poſition of their orbits; and as to taking their ſituations in many ſucceſſive oppoſitions of the planet, which is likewiſe another very eligible method, that muſt of courſe remain to be done at proper

per opportunities. The only way then left, was to take the situations of these satellites, in any place where I could ascertain them with some degree of precision, and to reduce them afterwards by computation to such other situations as were required for my purpose.

In January, February, and March, 1787, the positions were determined by causing the planet to pass along a wire, and estimating the angle a satellite made with this wire, by a high magnifying power; but then I could only use such of these situations where the satellite happened to be either directly in the parallel of declination, or in the meridian of the planet; or where, at least, it did not deviate above a few degrees from either of them; as it would not have been safe to trust to more distant estimations. In October I had improved my apparatus so far as to measure the positions by the same angular micrometer with which I have formerly determined the relative positions of double stars *.

In computing the periods of the satellites I have contented myself with synodical appearances, as the position of their orbits, at the time when the situations were taken from which these periods are deduced, was not sufficiently known to attempt a very accurate sidereal calculation. By six combinations of positions at a distance of 7, 8, and 9 months of time, it appears that the first satellite performs a synodical revolution round its primary planet in 8 days 17 hours 1 minute and 19,3 seconds. The period of the second satellite deduced likewise from four such combinations, at the same distance of time, is 13 days 11 hours 5 minutes and 1,5 seconds. The

* For a description of this instrument, see Phil. Trans. Vol. LXXI. p. 500. and Vol. LXXV. p. 46.

combinations of which the above quantities are a mean do not differ much among themſelves; it may therefore be expected that theſe periods will come very near the truth; and, indeed, I have for many months paſt been uſed to calculate the places of the ſatellites by them, and have hitherto always found them in the ſituations where theſe computations gave me reaſon to expect to ſee them.

The epochæ, from which aſtronomers may calculate the poſitions of theſe ſatellites, are October 19, 1787; for the firſt 19 h. 11′ 28″; and for the ſecond 17 h. 22′ 40″. They were at thoſe times 76° 43′ north-following the planet; which, as will be ſhewn in the ſequel, is the place of the greateſt elongation of the ſecond ſatellite; where, conſequently, its real angular ſituation is the ſame as the apparent one. And I have brought the firſt ſatellite to the ſame place, as hitherto there has not been time to diſcriminate the ſituation of its orbit from that of the ſecond.

The next thing to be determined in the elements of theſe ſatellites is their diſtance from the planet; and as we know that, when the periodical times are given, it is ſufficient to have the diſtance of one ſatellite in order to find that of any other, I confined my attention to the diſcovery of the diſtance of the ſecond. As ſoon as I attempted meaſures, it appeared, that the orbit of this ſatellite was ſeemingly elliptical; it became therefore neceſſary, in order to aſcertain its greateſt elongation, to repeat theſe meaſures in all convenient ſituations; the reſult of which was, that on the 18th of March, at 8 h. 2′ 50″, I found the ſatellite at the diſtance of 46″,46; this being the largeſt of all the meaſures I have had an opportunity of taking. Hence, by computation, it appears, that the

ſatellite's greateſt viſible elongation from its planet, at the mean diſtance of the Georgium ſidus from the earth, will be 44″,23.

It ought to be mentioned, that in the reduction of this meaſure I have uſed MAYER's tables for the ſun, and the tables publiſhed in the *Connoiſſance des Temps* of the year 1787, reduced to the time of Greenwich, for the Georgian planet.

Very poſſibly this diſtance might not be taken exactly at the time when the ſatellite happened to be at the vertex of the tranſverſe axis of its apparently elliptical orbit; but, from other meaſurements, we have reaſon to conclude, that it could not be far from that point. For inſtance, the 9th of November, at 15 h. 56′ 15″, by a mean of four good meaſures, the ſatellite was 44″,89 from the planet; which, by calculation, reduced to the ſame diſtance of the Georgian ſidus from the earth as the former, gives 41″,33. And likewiſe, the 19th of March, at 7 h. 45′ 59″, the diſtance meaſured 44″,24; which, computed as before, gives 42″,15. Now, we find, when the places are calculated in which the ſatellite happened to be at the times when theſe two meaſures were taken, that they fall on different ſides of the former meaſure, and alſo on oppoſite parts of the ſatellite's orbit; but that nevertheleſs they agree ſufficiently well with the poſition of the tranſverſe axis which we have adopted in the ſequel.

Admitting, therefore, at preſent, that the ſatellite moves in a circular orbit about its planet, we cannot be much out in taking the calculated quantity of 44″,23 for the true meaſure of its diſtance. And, having aſcertained this point, we calculate, by the law of KEPLER, and the aſſigned period of the firſt ſatellite, that its diſtance from the planet muſt be 33″,09. I ought however to remark, that, in this computation, a true ſidereal period ſhould have been uſed; but, as that cannot as

yet

yet be had, the trifling inaccuracy thence arifing may well be excuſed, till, at ſome future opportunity, we may be permitted to repeat theſe calculations in a more rigorous manner.

As we are now upon the ſubject of ſuch parts of the theory of planets as may be determined by calculation, it will not be amiſs to ſee how the quantity of matter and denſity of our new planet will ſtand, when compared with the tables that have been given of the ſame in the other planets; and in order to this, let us admit the following *data* as a foundation for our computation.

The parallax of the ſun 8″,63.

The parallax of the moon 57′ 11″.

Its ſidereal revolution round the earth 27 d. 7 h. 43′ 11″,6.

The mean diſtance of the Georgian planet from the ſun 19,0818.

The mean diſtance of its ſecond ſatellite from the planet 44″,23.

The periodical time of this ſatellite 13 h. 11 d. 5′ 1″,5.

Hence we find, that a ſpectator, removed to the mean diſtance of the Georgian planet from the earth, would ſee the radius of the moon's orbit under an angle of 27″,1866; and if 1, *d*, *t*, repreſent the quantity of matter in the earth, the diſtance of the moon, and its periodical time; M, D, T, be made to ſtand for the ſame things in our new planet and its ſecond ſatellite, we obtain, by known principles, $M = \frac{t^2 D^3}{T^2 d^3}$. And, conſequently, the quantity of matter in the Georgian planet is to that contained in the earth as 17,740612 to 1.

In order to caculate the denſity, I compare the mean of the four bright meaſures of the planet's diameter 3,″7975 to the

mean of the two dark ones 4″,295; as they are given in my Paper on the diameter and magnitude of the Georgium ſidus, printed in Volume LXXIII. of the Philoſophical Tranſactions, p. 9. 11, 12, 13. Whence we obtain another mean diameter 4″,04625; which is probably the moſt accurate of any that we have hitherto aſcertained. And let us ſuppoſe this meaſure to belong to the ſituations of the earth and of the new planet as they were at 10 o'clock, the 25th of October, 1782; which is about the middle of the ſeveral times when thoſe meaſures from which this is deduced were taken. Then, by the tables already referred to, we compute the diſtance of the two planets from the ſun and the angle of commutation; whence, by trigonometry, we find the diſtance of our new planet from the earth for the ſuppoſed 25th of October; and thence deduce its mean diameter, which is 3″,90554. This, when brought to what it would appear if it were ſeen from the ſun at the earth's mean diſtance, gives 1′ 14″,5246; which, compared with 17″,26, the earth's mean diameter, is as 4,31769 to 1. The Georgium ſidus, therefore, in bulk, is 80,49256 times as large as the earth; and conſequently its denſity leſs than that of the latter in the ratio of ,220401 to 1.

To theſe particulars, though many of them may be of no other uſe than merely to ſatisfy our curioſity, we may alſo add, that the force of gravity, on this planet's ſurface, is ſuch as will cauſe an heavy body to fall through 18,67308 feet in one ſecond of time.

It remains now only, in order to complete our general idea of the Georgian planet, to inveſtigate the ſituation of the orbits of its ſatellites. I have before remarked, that when I came to examine the diſtance of the ſecond, I perceived immediately that its orbit appeared conſiderably elliptical. This induced

me

me to attempt as many meaſures as poſſible, that I might be enabled to come at the proportion of the axes of the apparent ellipſis; and thence argue its ſituation. But here I met with difficulties that were indeed almoſt inſurmountable. The uncommon faintneſs of the ſatellites; the ſmallneſs of the angles to be meaſured with micrometers which required light enough to ſee the wires; the unwieldy ſize of the inſtrument, which, though very manageable, ſtill demanded aſſiſtant hands for its movements, and conſequently took away a great ſhare of my own directing power, a thing ſo neceſſary in delicate obſervations; the high magnifiers I was obliged to uſe by way of rendering the ſpaces and angles to be meaſured more conſpicuous; in ſhort, every circumſtance ſeemed to conſpire to make the caſe a deſperate one. Add to this, that no meaſure could poſſibly ſucceed which had not the moſt beautiful ſky in its favour; and we may eaſily judge how ſcarce the opportunities of taking ſuch meaſures muſt be in the variable climate of this iſland. As far then as a ſmall number of ſelect meaſures will permit, which, out of about twenty-one that were taken, amounts only to five, I ſhall enter into our preſent ſubject of the poſition of the ſecond ſatellite's orbit.

The following table contains in the firſt column the correct mean time when the meaſures were taken. The ſecond gives the quantity of theſe meaſures. In the third column are the ſame meaſures reduced to the mean diſtance of the Georgian planet from the earth. The fourth contains the calculated poſitions of the ſatellite as it would have appeared to be ſituated if it had moved in a circular orbit at rectangles to the viſual ray; and the degrees are numbered from the firſt obſervation ſuppoſed to have been at zero, and are carried round the circle from right to left.

March

	D.	H.	′	″	″	″	°	′
March	18	8	2	50	46,46	44,23	0	0
	19	7	47	59	44,24	42,15	26	28
	20	7	44	8	40,23	38,37	53	8
April	11	9	18	27	35,32	34,35	283	13
Nov.	9	15	56	15	44,89	42,88	199	59

In the uſe of this table I ſhall partly content myſelf with the conſtruction of a figure, and only apply calculation to the moſt material circumſtances. By the third column we ſee that 44″,23 is the greateſt, and 34″,35 the leaſt, diſtance of the ſatellite. Let therefore an ellipſis be drawn Tab. V. fig. 1. having the tranſverſe and conjugate diameters *cp* and *cv*, in the proportion of the above-mentioned meaſures. About the center *c*, with the radius *cp*, deſcribe the circle PSFN; and ſet off the points March 18, 19, 20. April 11. and Nov. 9. according to the tabular order of degrees, beginning at *p*, the ſuppoſed zero. From theſe points to the tranſverſe draw the ordinates March 19 *s*, 20 *t*, April 11 *x*, Nov. 9 *y*. Then, if the ſatellite moved in a circular orbit at rectangles to the viſual ray, we ſhould have ſeen it at the time given in the table, as the points are placed in the circumference of our circle; but, ſuppoſing the plane of the orbit inclined to the viſual ray, theſe points will be projected in the direction of the ordinates; and, falling on the places *p n m r o*, will form the ellipſis we have delineated. Now, on comparing the tabular meaſures of the third column with the diſtances of *p n m r* and *o* from the center *c*, we find, that they agree full as well as we could expect; and thus, as far as a few obſervations can do, theſe meaſures eſtabliſh the truth of the above hypotheſis.

That we may have a point in our ellipſis from which to depart, I ſhall have recourſe to two meaſures of poſitions. The firſt was taken October 14 d. 16 h. 28′ 42″, when the ſatellite was 66° 3′ ſouth-following the planet. In fig. 2. let *qmv* be a portion of an ellipſis, conſtructed on the ſemi-tranſverſe *cq*, and ſemi-conjugate *cv*, taken as 44,23 to 34,35; *q*MF an arch of a circle, deſcribed with the radius *cq* about the center *c*; *m* the ſituation of the ſatellite in its elliptical orbit, the 14th of October; A its apparent, and M its real place in the circle; F*c* the parallel of the planet. Then we ſhall have, by calculating from the known period, the arch *q*M 45° 17′; and FA, by obſervation, 66° 3′. But from the nature of the ellipſis, as V*c* is to *vc* ſo is M*n* (the tangent of the angle *qc*M to the radius *cn*) to *mn* (the tangent of the angle *qc*A to the ſame radius). Hence *qc*A is found 38° 7′; and therefore A*c*M 7° 10′. That is, when the angle of poſition was taken, the ſatellite appeared to be 7° 10′ leſs advanced in its orbit than it ſhould have done, owing to its motion in an orbit whoſe plane is inclined to the viſual ray. The meaſure therefore corrected, or rather reduced to the circle, inſtead of 66° 3′, will be 58° 53′ ſouth-following; to which, adding the calculated arch *q*A, and from the ſum deducting 90°, we have the poſition *qc*S with the meridian 14° 10′ on the ſouth-preceding ſide. In the ſame manner I proceed with the ſecond meaſure taken October 20 d. 16 h. 7′ 34″; when the ſatellite appeared to be 82° 12′ north-preceding the planet. Here the arch *q*M is 25° 21′, A*c*M 5° 9′; and the meaſure corrected 77° 3′ north-preceding, which gives the inclination of the axis to the meridian 12° 24′ on the north-following ſide. I have no reaſon to prefer either of the meaſures, and therefore take a mean of both, which is 13° 17′ from ſouth-preceding to

north-following the meridian, as probably nearest the truth; and this position of the axis we may suppose to belong to a time which is about the mean of those from which it has been deduced; or October, 17 d. 16 h.

We are, in the next place, to find the angle which the plane of the meridian made at that time with the plane of the orbit of the Georgian planet. To this end we calculate its longitude and latitude for the given time. Then, in fig. 3. where ☊ NE is part of the solstitial colure, *nc* a portion of the orbit, and ☊ *s* of the ecliptic; there is given the arch NE, 23° 28′; E*c*, 89° 27′ 49″,2 the complement of the planet's latitude; and the angle ☊ E*s*, 27° 0′ 52″,2, or planet's distance from Cancer. By these we find the angle E*c*N between the circle of latitude N*c* and the meridian E*c* 11° 11′ 41″. Now, let *c*, in fig. 4. be the place of the Georgian planet, and G☊*c* a part of its orbit; *e*☊*s* part of the ecliptic; N*c* the meridian; ☊ the place of the planet's ascending node; *pcq* the position of the axis of the apparently elliptical orbit of the second satellite; E*c*N the angle of position of the Georgium sidus. Then, by calculation, for the above-mentioned day we have ☊*c*, the planet's distance from the node on its orbit 44° 8′ 17″; *c*☊*s* the inclination of its orbit to the ecliptic 46′ 13″; and ☊*sc* a right-angle; whence E*c*☊ 90° 33′ 10″,1 the supplement of the angle *sc*☊ is found; from which, taking the angle of position N*c*E, before obtained, we have the remaining angle, N*c*G, 79° 21′ 29″,1; or inclination of the planet's orbit to the plane of the meridian, which was required.

From the proportion of the transverse *cp*, fig. 1. to the conjugate *cv*, we calculate the angle *vpc*, which may be either acute or obtuse. For here I must take notice, that observations cannot immediately determine whether the satellite, in passing from *p* through *n m v* to *q*, be in the farthest or nearest part of

its orbit; as we shall presently shew that this orbit is not in a situation to permit the satellite to suffer either eclipses or occultations for some time to come. The angle *vpc*, therefore, if the arch *pvq* be turned towards us will be 129° 2′ 46″,5; but, if directed the contrary way, 50° 57′ 13″,5. There is one circumstance which will bring on a discovery of this particular, without waiting for eclipses; for if the apparent ellipsis of the satellite's orbit should contract in a year or two, we may conclude this arch to lie towards the sun; if, on the contrary, it opens, we shall know that the satellite has passed through one of its nodes about eight or nine years ago; and that, therefore, we must not expect to see it eclipsed for more than thirty years to come.

Now, having already determined the position of the axis *pc* with respect to the meridian, by adding the angles N*cp* and N*c*G, fig. 4. we obtain *pcn*, 92° 38′ 29″,1; and having also now calculated the ambiguous angle *npc*, we may resolve the quadrantal triangle *pcn*, in which the angle *cnp* gives the inclination of the orbit of the satellite to the orbit of its planet, which will be 99° 39′ 48″,9, if the satellite be approaching to its ascending node; but 80° 20′ 11″,1, if it be lately past the descending one.

In the same triangle we find the side *nc*, which is either 50° 59′ 0″,8 or 129° 0′ 59″,2; and taking these quantities, increased by six signs, from the longitude of the planet in its orbit, gives the place of the satellite's ascending node upon the orbit of the planet, either 8 s. 6° 2′ 0″,3, if the preceding arch of the orbit *pmn*, be concave towards the sun; or 5 s. 18° 0′ 1″,9, if it be convex.

These elements obtained, we reduce them to the ecliptic by resolving the triangle ☊*nm*, in which we have *m*☊*n*, 46′ 13″;

$n\Omega$ the distance of the ascending node of the satellite from the descending node of the planet 6° 50′ 43″,8 or 84° 52′ 42″,1; and Ωnm, the inclination of the satellite's orbit to that of its planet 99° 39′ 49″,9 answering to the former, or 80° 20′ 11″,1 to the latter. In consequence of this resolution, we have the place of the ascending node of the satellite upon the ecliptic, $\left\{\begin{matrix}5\text{ s. }18^\circ\ 0'\ 3'',9\\8\quad 6\ 2\ 2\ ,3\end{matrix}\right\}$, and its inclination to the same $\left\{\begin{matrix}99^\circ\ 43'\ 53'',3\\81\quad 6\ 4\ ,4\end{matrix}\right\}$. The orbit being situated so, that when the planet will be in the ascending node of this satellite, which will happen about the year $\left\{\begin{matrix}1799\\1818\end{matrix}\right\}$, the northern half of it will be turned towards the $\left\{\begin{matrix}\text{East}\\\text{West}\end{matrix}\right\}$ at the time of its meridian passage.

In justice to the foregoing calculations I should add, that the result of them must be considerably affected by any small alteration in the measures upon which they are founded; the general theory, however, will certainly stand good, and a greater perfection in particulars could not have been obtained, unless I had waited some years, at least, in order to multiply good observations. But with objects that are out of the reach of common telescopes, and which therefore cannot be much attended to, even by our most assiduous astronomers, a general theory will perhaps nearly answer all the ends that may be required of it.

The measures of the distances were taken by a good parallel-wire micrometer, contrived so that one of the wires, which is moveable, can pass over the other; by which means central measures may be obtained with more accuracy than by allowing for

for the thickneſs of the wires, the aſcertaining of which is liable to ſome difficulties in other conſtructions; but here, as we can note the diviſions on the firſt appearance of light at either ſide of the fixed wire, when the moveable one paſſes over it backwards and forwards, we may very conveniently determine that part of the ſcale to which the zero ought to anſwer in central meaſures. The value of the ſcale was aſcertained by the tranſit of ſtars over the two wires opened to a certain number of diviſions, and a chronometer beating five times in two ſeconds of mean time; and in a number of ſeveral ſets of experiments, the mean of each ſeldom differed ſo much as the 500dth part of a ſecond of ſpace for each diviſion, and theſe are large enough to be ſub-divided and read off, with good exactneſs to tenths; and yet the ſpace anſwering to each part amounts only to 282 milleſimals of a ſecond. The meaſures of the diſtances alſo were as often repeated as the opportunities would permit, and a mean of them has been uſed.

The light of the ſatellites of the Georgian planet is, as we may well expect, on account of their great diſtance, uncommonly faint. The ſecond is the brighteſt of the two, but the difference is not conſiderable; beſides, we muſt allow for the effect of the light of the planet, which is pretty ſtrong within the ſmall diſtances at which they are revolving. I have ſeen ſmall fixed ſtars, as near the planets as the ſatellites, and with no greater light, which, on removal of the planet, ſhone with a conſiderable luſtre, ſuch as I had by no means expected of them. A ſatellite of Jupiter, removed to the diſtance of the Georgian planet, would ſhine with leſs than the 180th part of its preſent light; and may we not conclude, that our new ſatellites would be of a very conſiderable brightneſs if they were brought ſo near as the orbit of Jupiter, and thus appeared

180

180 times brighter than at preſent? Nay, this is only when we take both the planets at their mean diſtance; for, in their oppoſitions, a ſatellite brought from the ſuperior planet to the orbit of the inferior one, would reflect nearly 250 times the former light; from all which it is evident, that the Georgian ſatellites muſt be of a conſiderable magnitude.

If we draw together the reſults of the foregoing calculations into a ſmall compaſs, they will ſtand as follows:

The firſt ſatellite revolves round the Georgian planet in 8 days 17 hours 1 minute and 19 ſeconds.

Its diſtance is 33″.

And on the 19th of October, 1787, at 19 h. 11′ 28″, its poſition was 76° 43′ north-following the planet.

The ſecond ſatellite revolves round its primary planet in 13 days 11 hours 5 minutes and 1,5 ſeconds.

Its greateſt diſtance is 44″,23.

And on the 19th of October, 1787, its poſition at 17 h. 22′ 40″, was 76° 43′ north-following the planet.

Laſt year its leaſt diſtance was 34″,35; but the orbit is ſo inclined, that this meaſure will change very conſiderably in a few years, and by that alteration we ſhall know which of the double quantities put down for the inclination and node of its orbit are to be uſed.

The orbit of the ſecond ſatellite is inclined to the ecliptic $\left\{\begin{matrix} 99^\circ\ 43'\ 53,3'' \\ 81\ \ 6\ \ 4,4 \end{matrix}\right\}$.

Its aſcending node is in $\left\{\begin{matrix} 18 \text{ degrees of Virgo} \\ 6 \text{ degrees of Sagittarius} \end{matrix}\right\}$.

When the planet paſſes the meridian, being in the node of this ſatellite, the northern part of its orbit will be turned towards the $\left\{\begin{matrix} \text{Eaſt} \\ \text{Weſt} \end{matrix}\right\}$.

The

The ſituation of the orbit of the firſt ſatellite does not ſeem to differ materially from that of the ſecond.

We ſhall have eclipſes of theſe ſatellites about the year $\left\{\begin{matrix}1799\\1818\end{matrix}\right\}$, when they will appear to aſcend through the ſhadow of the planet almoſt in a perpendicular direction to the ecliptic.

The ſatellites of the Georgian planet are probably not leſs than thoſe of Jupiter.

The diameter of the new planet is 34217 miles.

The ſame diameter ſeen from the earth, at its mean diſtance, is 3″,90554.

From the ſun, at the mean diſtance of the earth, 1′ 14″,5246.

Compared to that of the earth as 4,31769 to 1.

This planet in bulk is 80,49256 times as large as the earth.

Its denſity as ,220401 to 1.

Its quantity of matter 17,740612 to 1.

And heavy bodies fall on its ſurface 18 feet 8 inches in one ſecond of time.

W. HERSCHEL.

Slough, March 1, 1788.

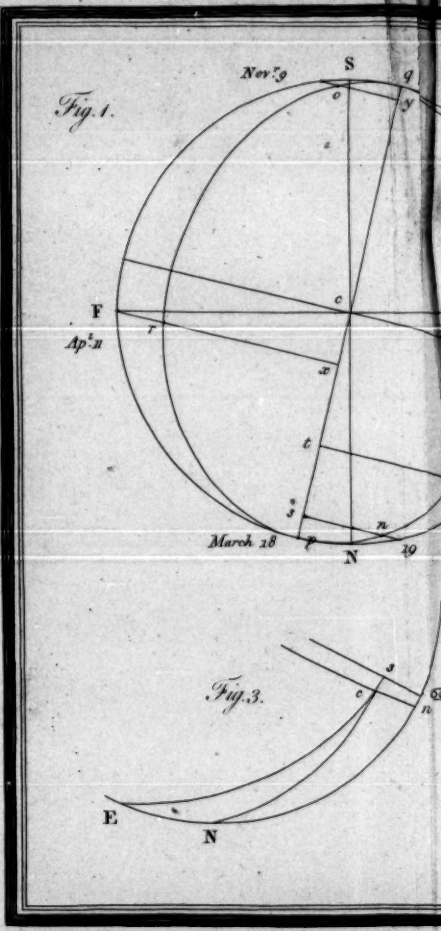
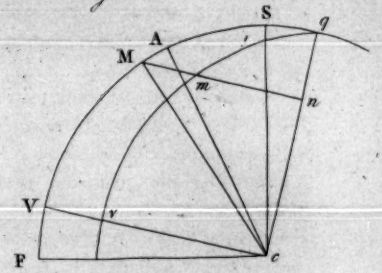


Fig. 1.
Nov.r 9
S
F
Ap.l n
March 18
N
19
Fig. 3.
E
N

Fig. 2.



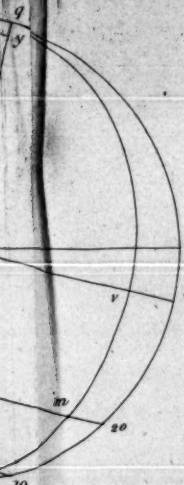
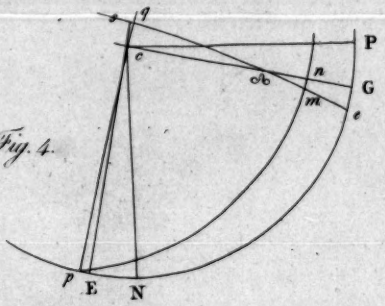


Fig. 4.



Bazire sc.

XXIII. *Experiments on the Formation of Volatile Alkali, and on the Affinities of the Phlogisticated and light Inflammable Airs. By* William Austin, *M. D. Fellow of the College of Physicians; communicated by* Charles Blagden, *M. D. Sec. R. S.*

Read May 29, 1787.

IN the former part of the year 1787, I undertook to examine the elastic fluid produced upon decomposing volatile alkali by the electric stroke, as first suggested by Dr. Priestley. Some alkaline air being thus decomposed, and all its inflammable part separated by combustion in glass vessels inverted in quicksilver, I observed a considerable remainder of phlogisticated air; and after many accurate experiments was fully convinced, that this phlogisticated air had made a part in the constitution of the alkali. This discovery induced me to make a variety of synthetical experiments on the phlogisticated and light inflammable airs, with the hopes of forming volatile alkali from its simple elements. In this undertaking I also derived much assistance from the ingenious labours of Dr. Priestley; who, in the course of his experiments, had been repeatedly struck with the smell of volatile alkali from substances not supposed to contain it, and had pointed out some important phænomena attending its production. But having acquired a more perfect knowledge of its constituent parts, I was enabled to produce volatile alkali in a more simple manner, and more demonstrative of its elements, than he had done.

I was not appriſed, when I had theſe experiments in hand, that the firſt object of my inquiry, the decompoſition of volatile alkali, and analyſis of its parts, had been ſucceſsfully treated by the celebrated M. BERTHOLLET; and that his obſervations had appeared in ROZIER's Journal ſome months before. To him therefore I give up that part of the ſubject; and ſhall confine myſelf in the following obſervations to ſome experiments on the formation of volatile alkali, and on the affinities of the phlogiſticated and light inflammable airs, which have not, I believe, been hitherto taken notice of.

Firſt, I endeavoured to combine the phlogiſticated and light inflammable airs, by mixing them together in various proportions in their elaſtic ſtate, and adding to them ſuch ſubſtances as I thought likely to promote their uniting and forming an alkali. With this view, I threw up to the mixture of theſe airs, marine acid air, the marine and vitriolic acids, to which I alſo joined alkaline air. I tried the effect of cold upon theſe mixtures, by applying to the tubes containing them cloths moiſtened with ether. I even paſſed the electric ſpark repeatedly through them, though with little probability of ſucceſs. Laſtly, I decompoſed alkaline air, and tried to reunite the identical parts which formed it by ſimilar additions; but I could not perceive, that in any inſtance volatile alkali was produced from its two conſtituent parts mixed together in their ſimple aëriform ſtate.

Yet it is well known, that theſe two bodies unite very readily, when they are not in an elaſtic ſtate. An unexpected appearance of volatile alkali had been obſerved by Dr. PRIESTLEY and Mr. KIRWAN before we were acquainted with its conſtitution, and by M. HAUSSMAN ſince this diſcovery of M. BERTHOLLET. An experiment was exhibited before ſeveral Gentlemen at Sir JOSEPH BANKS's Houſe, ſome years ago, in

in which the quantity of volatile alkali produced is very remarkable. In this experiment a few ounces of powdered tin are moiſtened with ſome moderately ſtrong nitrous acid, and after they have ſtood together a minute or two, about half an ounce of fixed alkali is mixed with them. A very pungent ſmell of volatile alkali is immediately perceived. The experiment ſucceeds equally, if lime be uſed inſtead of fixed alkali. Any perſon, who moiſtens a drachm or two of filings of zinc with a ſolution of cupreous nitre, and, after they begin to act on each other, adds to them a little ſalt of tartar, will find volatile alkali to be produced. Nitrous acid, or cupreous nitre, mixed with iron filings, ſulphur, and a little water, and kept in a cloſe veſſel for ſome hours, yields a ſmell of volatile alkali; and if a piece of paper, ſtained with a vegetable blue ſubſtance, be thrown into the veſſel, it will, in a ſhort time, be turned to a green colour. In each of theſe experiments the nitrous acid and the water are decompoſed. Dephlogiſticated air from each of them combines with the metal, and their other conſtituent parts, the phlogiſticated air of the acid, and inflammable air of the water, being diſengaged at the ſame inſtant, unite and form volatile alkali. Many other ſimilar experiments might be mentioned; but theſe are abundantly ſufficient to prove, that if phlogiſticated and light inflammable air be preſented to each other at the inſtant of their ſeparation from ſolid or liquid ſubſtances, and before their particles have receded from each other, they readily combine and generate volatile alkali.

That theſe two ſubſtances do not combine in their elaſtic ſtate, ſeems to be owing principally to the inflammable air. When theſe two airs combine, it ſeems neceſſary that they part with a certain quantity of that fire to which they owe their elaſticity; and that, unleſs their attraction to each

other exceed their attraction to fire, they will not unite. Even when they are combined in the form of volatile alkali, if heat be applied, they immediately recede from each other, and the alkali is decompoſed. When they are not in an aëriform ſtate their attraction to each other is greater, on account of the proximity of their parts; it is then ſuperior to their attraction to fire, and therefore they combine; but when their particles have receded from each other, as in the aëriform ſtate, their attraction to each other is ſo diminiſhed by the diſtance of their parts, that their attraction to fire, which is uniform, prevails, and keeps them in a ſeparate ſtate. The ſpecific gravity of inflammable air being eleven times leſs than that of phlogiſticated air, the diſtance of its particles muſt be greater than the diſtance of the particles of phlogiſticated air in the proportion of $\sqrt[3]{11}$ to 1, if the elementary particles of the two airs be of equal magnitude; and its effect, on this account, in diminiſhing attraction muſt be greater than that of phlogiſticated air, in the proportion of, or more probably as the ſquares of, thoſe numbers.

Whether it be admitted, that fire thus combines with other ſubſtances, and is ſeparated from them by their mutual attractions, according to the general law, is not further material to the preſent enquiry, than as it accounts for a circumſtance which ſeems to be eſtabliſhed by the following experiments; namely, that the combination of the phlogiſticated and inflammable airs, and the formation of volatile alkali, depends chiefly, if not altogether, on the approximation of the parts of inflammable air, when phlogiſticated air is preſented to them.

Into a cylindrical glaſs tube, filled with, and inverted in, quickſilver, I introduced ſome phlogiſticated air, and afterwards ſome iron filings moiſtened with diſtilled water. By this

this arrangement light inflammable air, which is given out from water in contact with iron filings, meeting with phlogisticated air at the instant of its extrication, combines with it, and forms volatile alkali. In order to detect the minute quantities of volatile alkali, which were thus generated, I fixed to the inside of the glass tube a small piece of paper, stained with the rind of the blue raddish. The vegetable blue was in twenty-four hours changed to a green colour. As an additional proof of the production of volatile alkali, I kept in the same tube some paper, which had been dipped in a solution of cupreous nitre, expecting to see its colour converted from green to blue, by the alkali which was to be produced. The green paper became gradually paler, and in a few days the blue colour appeared. This experiment affords a very satisfactory demonstration of the formation of volatile alkali. Water and iron filings mixed together yield inflammable air; but if this be given out in contact with phlogisticated air, volatile alkali is produced. In these circumstances a double attraction takes place: one part of the water is attracted by the iron; the other is attracted by the phlogisticated air; and the water seems by these compound affinities to be much more rapidly decomposed, than when iron and water are mixed by themselves.

Volatile alkali is formed in a very few hours, if nitrous air be used instead of the phlogisticated, all other circumstances remaining as in the former experiment. When I have made use of nitrous air not well freed from its acid, by which the vegetable blue colour has been turned red, a sufficient quantity of alkali has been generated in twenty-four hours to change it to a green. If iron filings and water be exposed to nitrous air for a considerable time, the nitrous air is so altered that a candle burns in it with increased brightness, as was observed by

Dr.

Dr. PRIESTLEY. This change is accounted for by the formation of the alkali, which depriving the nitrous air of its phlogisticated part leaves a greater proportion of dephlogisticated air.

This experiment also succeeds in atmospheric air, though a longer time is necessary to produce a sensible alteration in the colours employed as tests of the alkali; but the change is very evident in a day or two. Hence we may conclude, that whenever iron rusts in contact with water in the open air, or in the earth, volatile alkali is formed. Phlogisticated air is present in all parts of the terraqueous globe, and operations are constantly going on, by which inflammable air is separated from water, and perhaps from other bodies. Thus we may account for the frequent appearance of volatile alkali in the earth, particularly where inflammable matters abound, among coals and volcanic productions, as also in animal and vegetable substances.

When iron, water, and sulphur act upon each other in atmospheric air, volatile alkali is produced. The eudiometer recommended by SCHEELE is, for this reason, incorrect. Some phlogisticated air disappears, and volatile alkali is formed. This method therefore seems to have misled that great chemist in his analysis of the atmosphere, and induced him to suppose, that the quantity of phlogisticated air in the atmosphere is only 2¾ times that of dephlogisticated air.

There is a combination of light inflammable air with sulphur forming hepatic air. It has been observed by the celebrated Mr. KIRWAN, that if nitrous air be mixed with hepatic air volatile alkali will be formed. I have often repeated this experiment, and marked the formation of the volatile alkali by the change of the vegetable blue to a green colour. In hepatic

air

air the parts of inflammable air are brought nearer to each other than they are in their ſimple aëriform ſtate*, and therefore the phlogiſticated air of the nitrous air combines with them, and generates volatile alkali.

From all theſe experiments it follows, that whether phlogiſticated air be in a ſtate of purity, or mixed with dephlogiſticated air, as in the atmoſphere, or combined with it as in nitrous air, it will in either caſe unite with the gravitating matter of light inflammable air, provided this ſubſtance be preſented to it in a ſtate of condenſation; but if the circumſtances be reverſed, the ſame combination does not take place. No union is formed between inflammable air and the phlogiſticated part of nitrous air, even though marine acid be added, which, by its attraction to dephlogiſticated air, would contribute to decompoſe the nitrous air, and by its attraction to volatile alkali would tend to unite its conſtituent parts: or if to light inflammable air we add nitrous air and iron filings, no combination enſues; though it has been often obſerved, that volatile alkali is readily generated, when nitrous air is preſented to the inflammable at the inſtant of its extrication from water and iron.

The proportions of the phlogiſticated and inflammable airs in volatile alkali, as diſcovered by calculation, approach very near

* Since theſe experiments were made, I have found that this is not the caſe. The electric ſpark decompoſes hepatic air, and leaves a quantity of inflammable air equal in bulk to the hepatic air very nearly. However, as the inflammable air leaves the ſulphur upon the application of the electrical ſpark, it ſhould ſeem, that the proper matter of inflammable air is more diſpoſed to combine with fire than with ſulphur; which may be the reaſon why hepatic air is decompoſed by nitrous air, while pure inflammable air is not affected by it.

to the result of M, BERTHOLLET's experiments. If we take the specific gravities of these airs, given in Mr. KIRWAN's late publication,

100 cubic inches contain 18,16 grains of alkaline air.
—— —— —— 30,535 — of phlogisticated air.
—— —— —— 2,613 — of inflammable air.

According to M. BERTHOLLET alkaline air is expanded upon decomposition from 1,7 to 3,3. Its specific gravity after decomposition must therefore be lessened in the same proportion; and 100 cubic inches will be found to contain only 9,355 grains of alkaline air thus expanded. In what proportion must the phlogisticated and inflammable airs be, in order to form a mixture of this specific gravity?

Let x represent the number of grains of phlogisticated air in 100 cubic inches of the mixture: then $9{,}355 - x$ will express the number of grains of inflammable air. As the weight of one cubic inch is to a cubic inch, so will the weight of either air in the mixture be to the cubic inches of that air in the mixture; and therefore $\frac{30{,}535}{100}$ the weight of a cubic inch of phlogisticated air, or which is the same, ,30535 shall be to 1, as x is to $\frac{x}{,30535}$ which must be the number of cubic inches of phlogisticated air in 100 cubic inches of the mixture; and the weight of a cubic inch of inflammable air, that is, $\frac{2{,}613}{100}$, or ,02613 : 1 :: 9, 355 $- x$: $\frac{9{,}355 - x}{,02613}$ the cubic inches of inflammable air in 100 cubic inches of the mixture. Thus we have an expression for the cubic inches of each air; these two quantities taken together are equal to 100 cubic inches by supposition. We have then,

4 $\frac{x}{,30535}$

$$\frac{x}{,30535} + \frac{9,355-x}{,02013} = 100;$$

$$x + \frac{2,85654925-,30535x}{,02613} = 30,535;$$

$$,02613x + 2,85654925 - ,30535x = ,79787955;$$

$$,27922x = 2,05866970$$

$x = 7,373$ the number of grains of phlogisticated air in 100 cubic inches, or in 9,355 grains of the mixture; and $9,355 - 7,373 = 1,982$, the grains of inflammable air.

Now $7,373 : 1,982 :: 121 : 32$; the quantity of phlogisticated air is to that of inflammable, as 121 to 32.

According to M. BERTHOLLET's experiments, the quantity of phlogisticated is to that of inflammable air, as 121 : 29. This is not very wide of calculation. If we consider the great difficulty of obtaining these specific gravities with exactness, we must be pleased to find so near a concurrence, and place more confidence in experiments on the specific gravities and combinations of aëriform bodies, than has generally been given them. M. BERTHOLLET's experiments come within $\frac{1}{10}$ of calculation; and this difference will be diminished by two-thirds, if we take the specific gravities of the phlogisticated and inflammable airs in the proportion of 11 to 1, as he has done, instead of Mr. KIRWAN's proportion, which I have followed in this calculation.

XXIV. *Some Properties of the Sum of the Divisors of Numbers.* By Edward Waring, *M. D. F. R. S.*

Read May 29, 1788.

1. LET the equation $\overline{x-1}\,.\,\overline{x^2-1}\,.\,\overline{x^3-1}\,.\,\overline{x^4-1}\,.\,\overline{x^5-1}\,.\,\ldots\,\overline{x^n-1} = x^b - px^{b-1} + qx^{b-2} - rx^{b-3} + sx^{b-4} - \&c. = x^b - x^{b-1} - x^{b-2} + x^{b-5} + x^{b-7} - x^{b-12} - x^{b-15} + x^{b-22} + x^{b-26} - x^{b-35} - x^{b-40} + x^{b-51} + x^{b-57} - \&c. \ldots x^{b-v} \pm \&c. = A = 0$. The signs + and − proceed alternately by pairs unto the term x^{b-v}. The co-efficients of all the terms to the above mentioned (x^{b-v}) will be $+1$, -1 or 0; they will be $+1$, when multiplied into x^{b-v}, where $v = \frac{3z^2+z}{2}$ or $= \frac{3z^2-z}{2}$, and z an even number; but -1, if z be an uneven number; in all other cases they will be $= 0$.

The numbers 1, 2, 5, 7, 12, 15, 22, 26, 35, 40, &c. subtracted from b may be collected from the addition of the numbers 1, 1, 3, 2, 5, 3, 7, 4, 9, 5, 11, 6, &c. which consist of two arithmetical series 1, 3, 5, 7, 9, 11, &c. 1, 2, 3, 4, 5, 6, 7, &c. intermixed.

2. The sum of any power (m) of each of the roots in the equation $A = 0$ will be $S(m)$, where $S(m)$ denotes the sum of all the divisors of the number m, if m be not greater than n.

Cor. Hence (by the rule for finding the sum of (m) powers of each of the roots from the sum of the inferior powers and co-efficients of the given equation) may be deduced $S(m) =$

$pS(m-1)-qS(m-2)+rS(m-3)-sS(m-4)+tS(m-5)-$ &c. $= S(m-1)+S(m-2)-S(m-5)-S(m-7)+S(m-12)+S(m-15)-S(m-22)-S(m-26)+$ &c. which is the property of the sum of divisors invented by the late M. EULER.

Cor. By substituting for $S(m-1)$, $S(m-2)$, &c. their values $S(m-2)+S(m-3)-S(m-6)-S(m-8)+$ &c., $S(m-3)+S(m-4)-S(m-7)-S(m-9)+$ &c. &c. in the given equation $S(m) = S(m-1)+S(m-2)-S(m-5)-S(m-7)+$ &c. may be acquired an expression for the sum $S(m)$ in terms of the sums of the divisors of numbers less than $m-1$, $m-2$, &c. — the same method may be used for a similar purpose in some of the following propositions.

Cor. By the rule for finding the sum of the contents of every (m) roots from the sums of the powers of each of the roots may be deduced the equation $= 1 . 2 . 3 . 4 \ldots m$, or

$$0 = 1 - m \cdot \frac{m-1}{2} S(2) + m \cdot \overline{m-1} \cdot \frac{m-2}{3} S(3)$$
$$- m \cdot \overline{m-1} \cdot \overline{m-2} \cdot \frac{m-3}{4} S(4) + \&c.$$
$$\ldots$$
$$+ m \cdot \overline{m-1} \cdot \frac{m-2}{2} \cdot \frac{m-3}{2^2} S((2))^2 - \&c.$$

in which the sum of the divisors of any number m is expressed by the sums of the divisors of the inferior numbers $m-1$, $m-2$, &c. and their powers. If v be an even number, then $= 1 . 2 . 3 . . m$ will have the same sign as the co efficient; if uneven, the contrary; but if the co-efficient $= 0$, then will the content $1 . 2 . 3 . . m$ vanish. The law of this series is given in the Meditationes Algebraicæ.

3. Let H be the number of different ways by which the sum of any two numbers 1, 2, 3, 4, . . . $m-2$, $m-1$, can become $= m$; H′ the number of ways by which the sum of any three of the above-mentioned numbers can make m; H″, H‴, H⁗, &c. the number of ways by which the sum of any four, five, six,

&c. of the above-mentioned numbers is $= m$ respectively; then will $1 - H + H' - H'' + H''' - \&c. = \pm 1$ or 0. Let $m = \frac{3z^2 \pm z}{2}$, and it will be $+1$ or -1, according as z is an odd or even number, in all other cases it will be $= 0$.

PART II.

1. Let the equation be $\overline{x-1} \cdot \overline{x^2-1} \cdot \overline{x^3-1} \cdot \overline{x^5-1} \cdot \overline{x^7-1} \cdot \overline{x^{11}-1} \cdot \overline{x^{13}-1} \cdot \overline{x^{17}-1} \ldots \overline{x^n-1}$. &c. $= x^{b'} - px^{b'-1} + qx^{b'-2} - rx^{b'-3} + sx^{b'-4} - \&c. = x^{b'} - x^{b'-1} - x^{b'-2} + x^{b'-4} + x^{b'-8} - x^{b'-10} - x^{b'-11} + x^{b'-12} + x^{b'-16} - x^{b'-17} - x^{b'-19} + x^{b'-20} - x^{b'-23} + 2x^{b'-24} - x^{b'-26} - x^{b'-27} + x^{b'-28}$, &c. $= A' = 0$; the sum of any power (m) of each of the roots in the equation $A' = 0$ will be $S'(m)$, where $S'(m)$ denotes the sum of all the prime divisors of the number m, and m is not greater than n.

Cor. Hence, by the rule before-mentioned $S'(m) = S'(m-1) + S'(m-2) - S'(m-4) - S'(m-8) + S'(m-10) + S'(m-11) - S'(m-12) - S'(m-16) + S'(m-17) + S'(m-19) - S'(m-20) + S'(m-23) - 2S'(m-24) + S'(m-26) + S'(m-27) - S'(m-28) + S'(m-29)$, &c.

If in this, or the preceding, or subsequent analogous cases $S(m-r)$, or $S'(m-r)$, or $S'(m-r)$, becomes $S(o)$, or $S'(o)$, or $S'(o)$; for $S(o)$, or $S'(o)$, or $S'(o)$, always substitute r.

Cor. Let L be the co-efficient of the term $x^{b'-n}$; then, by the above-mentioned series contained in the Meditationes Algebraicæ, will $1 \cdot 2 \cdot 3 \cdot 4 \ldots m \times L = 1 - m \cdot \frac{m-1}{2} S'(2) + m \cdot \overline{m-1} \cdot \frac{m-2}{3} \times S'(3) - m \cdot \overline{m-1} \cdot \overline{m-2} \cdot \frac{m-3}{4} \times S'(4) + \&c.$ $+ m \cdot \overline{m-1} \cdot \overline{m-2} \cdot \frac{m-3}{8} \times S'((2))^3 - \&c.$

$-$ &c. be an equation, which expresses a relation between the prime divisors of the numbers 1, 2, 3, 4 . . . $m-1$, m, and their powers.

Cor. The co-efficient L = the difference between the two respective numbers of different ways that m can be formed adding the prime numbers 1, 2, 3, 5, 7, 11, 13, 19, &c. the one with, and the other without, 2.

PART III.

1. Let an equation $\overline{x^{\alpha}-1} \cdot \overline{x^{\beta}-1} \cdot \overline{x^{\gamma}-1} \cdot \overline{x^{\delta}-1} \times$ &c. $= x^{b} - px^{b-1} + qx^{b-2} - rx^{b-3} +$ &c. $= 0$; then will the sum of the (m) powers of each of its roots be the sum of all the divisors of m, that can be found amongst the numbers α, β, γ, δ, &c.

2. The co-efficient of the term x^{b-m} will be the difference between the two respective numbers of different ways, that the number (m) can be formed from the addition of the numbers α, β, γ, δ, &c.; the one containing in it an odd number of the even numbers contained in α, β, γ, δ, &c.; the other not.

PART IV.

1. Let $\overline{x^{l}-1} \cdot \overline{x^{2l}-1} \cdot \overline{x^{3l}-1} \cdot \overline{x^{4l}-1} \ldots\ldots \overline{x^{ml}-1}$, &c. $= x^{b} - px^{b-l} + qx^{b-2l} - rx^{b-3l} +$ &c. $= x^{b} - x^{b-l} - x^{b-2l} + x^{b-5l} + x^{b-7l} - x^{b-12l} - x^{b-15l} +$ &c. $= B = 0$, of which equation all the co-efficients are the same as in case the first, and consequently $= \pm 1$ or 0 to the term (x^{b-ml}).

2. The sum of any power $l \times m$ of each of the roots of the equation $B = 0$ will be $S^{l}(m)$, where $S^{l}(m)$ denotes the sum of the divisors of m, which are divisible by l.

Cor.

Cor. Hence $S^1(m) = S^0(m-l) + S^0(m-2l) - S^0(m-5l) - S^0(m-7l) + S^0(m-12l) + S^0(m-15l) - S^0(m-22l) - S^0(m-26l) +$ &c.; the law of the series has been given in Case 1.

Cor. The sum of all the divisors of m not divisible by $l = S(m) - S^1(m) = S(m-1) - S^1(m-l) + (S(m-2) - S^1(m-2l)) - (S(m-5) - S^1(m-5l)) - (S(m-7) - S^1(m-7l)) +$ &c.

A similar rule may be predicated of the sum of the divisors not divisible by the numbers $a, b, c, d,$ &c: for the sum of the divisors of the number (m) divisible by $a, b, c, d, e,$ &c., where $a, b, c, d, e,$ &c. are prime to each other $= (S^a(m) + S^b(m) + S^c(m) + S^d(m) + S^e(m) +$ &c.)) $- ((S^{a\times b}(m) + S^{a\times c}(m) + S^{a\times d}(m) + S^{a\times e}(m) + S^{b\times c}(m) + S^{b\times d}(m) + S^{b\times e}(m) + S^{c\times d}(m) + S^{c\times e}(m) +$ &c.)) $+ (S^{a\times b\times c}(m) + S^{a\times b\times d}(m) + S^{a\times b\times e}(m) + S^{a\times c\times d}(m) + S^{a\times c\times e}(m) + S^{a\times d\times e}(m) + S^{b\times c\times d}(m) + S^{b\times c\times e}(m) +$ &c.)) $- ((S^{a\times b\times c\times d}(m) + S^{a\times b\times c\times e}(m) + S^{a\times b\times d\times e}(m) + S^{a\times c\times d\times e}(m) + S^{b\times c\times d\times e}(m)$ &c.)) $+ (S^{a\times b\times c\times d\times e}(m)$ &c.)) $-$ &c. $= l =$ the sum of all the divisors of m . . . . divisible by $a, b, c, d, e,$ &c. respectively added together, $-$ the sum of all the divisors of m divisible by the products $(ab, ac, bc,$ &c.) of any two of the quantities $a, b, c, d,$ &c. $+$ the sum of all the divisors of m divisible by the contents $(abc, abd, acd, bcd,$ &c.) of every three of the quantities $a, b, c, d,$ &c. $-$ the sum of all the divisors of m divisible by the contents of every four of the abovementioned quantities $a, b, c, d,$ &c. $+$ and so on, and consequently $S(m) - C$ is the sum required.

The principles given in the former parts may be applied to this, and extended to equations of which the factors have the formula $x^a = k$; and from the sum of the inferior powers of each of the roots, and the co-efficients may be collected the sum of the superior; the same may be performed by the co-efficients only, &c.

PART

PART V.

1. $S(\alpha\beta) = \alpha \times S(\beta)$ + sum of all the divisors of β not divisible by $\alpha = \beta \times S(\alpha)$ + sum of all the divisors of α not divisible by β.

2. $S'(\alpha \times \beta) = \alpha \times S'(\beta)$ + sum of all the divisors of β divisible by l but not by $\alpha =$ &c.

3. $S(\alpha \times \beta \times \gamma \times \delta \times$ &c.$) = \alpha \times S(\beta \times \gamma \times \delta \times \varepsilon,$ &c.$)$ + sum of all the divisors of $\beta \times \gamma \times \delta \times \varepsilon,$ &c. not divisible by $\alpha = \alpha \times \beta \times S(\gamma \times \delta \times \varepsilon,$ &c.$)$ + sum of all the divisors of $\beta \times \gamma \times \delta \times \varepsilon,$ &c. not divisible by α + $\alpha \times$ sum of all the divisors of $\gamma \times \delta \times \varepsilon,$ &c. not divisible by $\beta = \alpha \times \beta \times \gamma \times S(\delta \times \varepsilon,$ &c.$)$ + sum of all the divisors of $\beta \times \gamma \times \delta \times \varepsilon,$ &c. not divisible by $\alpha + \alpha \times$ sum of all the divisors of $\gamma \times \delta \times \varepsilon,$ &c. not divisible by $\beta + \alpha \times \beta \times$ sum of all the divisors of $\delta \times \varepsilon,$ &c. not divisible by $\gamma = \alpha \times \beta \times \gamma \times \delta \times S(\varepsilon,$ &c.$)$ + sum of all the divisors of $\beta \times \gamma \times \delta \times \varepsilon,$ &c. not divisible by $\alpha + \alpha \times$ sum of all the divisors of $\gamma\delta\varepsilon,$ &c. not divisible by $\beta + \alpha \times \beta \times$ sum of all the divisors of $\delta\varepsilon,$ &c. not divisible by $\gamma + \alpha \times \beta \times \gamma \times$ sum of all the divisors of $\varepsilon,$ &c. not divisible by $\delta =$ &c. The law of the series is manifest. The letters α, β, γ, δ, &c. which are not contained between the parentheses, denote prime numbers.

Cor. If some of the letters α, β, γ, δ, &c. be substituted for others, and others for them, the equations resulting will be just, and consequently many new equations may be deduced.

If in the preceding equations for S be wrote S′, and for the sum of all the divisors of a certain quantity not divisible by a prime number (α, or β, or γ, &c.) be wrote the sum of all the

the divisors of that quantity not divisible by the same prime number, but divisible by l; the propositions resulting will be true.

These equations may be applied to the equations given in the preceding parts, and from thence many others deduced.

XXV. *Experiments on the Production of artificial Cold. By Mr.* Richard Walker, *Apothecary to the* Radcliffe Infirmary *at* Oxford. *In a Letter to* Henry Cavendish, *Esq. F.R.S. and A.S.*

Read June 5, 1788.

THE Royal Society having been pleased to insert, among their Transactions for last year, an account of some experiments of mine, relating to the production of artificial cold, transmitted in a letter from Dr. Brunoro, I am induced to mention a few I have made since.

Your zealous attention to this subject, under whose auspices this, as well as other branches of natural philosophy, hath received considerable improvement, will, I hope, apologize for the liberty I have taken in addressing myself to you, especially since any new and useful facts I may have ascertained are principally owing to those endeavours your excellent Papers have incited in me.

My most powerful frigorific mixture is the following:

Of strong fuming nitrous acid, diluted with water (rain or distilled water is best) in the proportion of two parts of the former to one of the latter, each by weight, well mixed, and cooled to the temperature of the air, three parts; of vitriolated natron (Glauber's salt) four parts; of nitrated ammonia (nitrous ammoniac) three and a half parts, each by weight, reduced separately to fine powder: the powdered vitriolated

natron is to be added to the diluted acid, the mixture well ſtirred, and immediately afterward the powdered nitrated ammonia, again ſtirring the mixture: to produce the greateſt effect, the ſalts ſhould be procured as dry and tranſparent as poſſible, and uſed freſhly powdered. Theſe ſeem to be the beſt proportions when the temperature of the air and ingredients is + 50°; as the temperature at ſetting out is higher or lower than this, the quantity of the diluted acid will evidently require to be proportionably diminiſhed or increaſed. This mixture is but little inferior to one made by diſſolving ſnow in nitrous acid, for it ſunk the thermometer from + 32° to − 10°; perhaps it may be poſſible to reduce the ſalts to ſo fine a powder as to make it equal. In this laſt experiment the diluted acid was equal in quantity to the vitriolated natron, being four parts each, the nitrated ammonia three and a half as before. A powder compoſed of muriated ammonia (crude ſal ammoniac) five parts, nitrated kali (nitre) four parts, mixed, may be ſubſtituted in the ſtead of nitrated ammonia, with nearly equal effect, and in the ſame proportion.

Cryſtallized nitrated ammonia, reduced to very fine powder, ſunk the thermometer, during its ſolution in rain-water, forty-eight degrees, from + 56°, the temperature of the air and materials, to + 8°; and when evaporated gently to dryneſs, and finely powdered, it ſunk the thermometer forty-nine degrees, to + 7°, the temperature of the air and materials being as before at + 56°: therefore, in this ſalt (which produces, as appears above, much greater cold during ſolution in water, than any other hitherto known) the water of cryſtallization is not in the leaſt conducive to that effect. I expected, that by diluting the ſtrong nitrous acid to the proper ſtrength with ſnow, inſtead of water, by which its temperature would be much reduced,

reduced, and then adding the ſalts, a much greater degree of cold might be produced; but, by various diverſified trials, I found but little advantage gained; I ſhall therefore forbear mentioning the particulars. In the courſe of this winter, ſome diluted nitrous acid, in a wide-mouthed phial, was immerſed in a freezing mixture; when cooled to about $-32°$, it froze intirely to the conſiſtence of an ointment, when the thermometer ſuddenly roſe to $-2°$; on adding ſome ſnow that lay by me, it became again liquid, and the mercury ſunk into the bulb of a thermometer graduated to $-76°$: I know not its exact ſtrength; but by the effect imagine it might correſpond nearly with that which is capable of the eaſieſt point of ſpirituous congelation. Cold, I have found, may be produced by the union of ſuch ſalts as on mixing are decompoſed, and become liquid or partially ſo. The mineral alkali produces this effect with all the ammoniacal ſalts; but with nitrated ammonia to a conſiderable degree. The mineral alkali added in powder to nitrous acid, diluted as above, ſunk the thermometer twenty-two degrees only, from 55° (temperature of air and materials) to 31°. This ſalt contains nearly as much water of cryſtallization as vitriolated natron, and produces more cold during ſolution in water than that ſalt. The reaſon why it produces leſs when added to acid than the neutral ſalt does, is perhaps ſufficiently evident. I have obſerved the thermometer to be ſtationary, or even to riſe, during the violent effervescence produced on mixing thoſe materials, and to ſink as ſoon as that ceaſed.

Vitriolated natron diſſolved indifferently in rectified ſpirit of wine, and produced neither heat or cold; the diſpoſition to produce cold, during its ſolution, being perhaps exactly counteracted by the tendency which the diſſolved ſalt hath in

uniting with the ſpirit to produce heat. Vitriolated magneſia (a ſalt very ſimilar to vitriolated natron) during ſolution in the diluted nitrous acid, produced nearly as much cold as that ſalt: the ſmall difference there is between them, as to this effect, may be owing to the former containing rather leſs water in its cryſtals.

Vitriolated natron, liquified by heat, was ſet to cool; when its temperature was reduced to 70°, it became ſolid, and the thermometer immediately roſe to 88° (eighteen degrees) its freezing point. Does not the quantity of ſenſible heat evolved by this ſalt, in becoming ſolid, indicate its great capacity for heat, in returning to a liquid ſtate, and conſequently account in a great meaſure for its producing ſuch intenſe cold during ſolution in the diluted mineral acids? Two ſalts, vitriolated argillaceous earth (alum) and tartarized natron (Rochelle ſalt), each contain nearly as much water of cryſtallization as vitriolated natron; but produced neither of them any conſiderable effect during ſolution in the diluted nitrous acid: the latter made the thermometer riſe: neither did their temperatures increaſe, like that ſalt, in changing from a liquid to a ſolid ſtate.

From the obvious application of artificial frigorific mixtures to uſeful purpoſes, in hot climates eſpecially, where the inhabitants ſcarcely know by the ſenſe of feeling winter from ſummer, it may not be amiſs to hint at the eaſieſt and moſt œconomical method of uſing them. For moſt intentions, perhaps, the following cheap one may be ſufficient: of ſtrong vitriolic acid, diluted with an equal weight of water, and cooled to the temperature of the air, any quantity; add to this an equal weight of vitriolated natron in powder: this is the proportion when the temperature ſet out with is + 50°,

and will ſink the thermometer to 5°; if higher, the quantity of the ſalt muſt be proportionably increaſed. The obvious and beſt method of finding the neceſſary quantity of any ſalt to produce the greateſt effect, by ſolution in any liquid, at any given temperature, is by adding it gradually until the thermometer ceaſes to ſink, ſtirring the mixture all the while.

If a more intenſe cold be required, double aqua fortis, as it is called, may be uſed; vitriolated natron, in powder, added to this, produces very nearly as much cold as when added to the diluted nitrous acid: it requires a rather larger quantity of the ſalt, at the temperature of + 50°, about three parts of the ſalt to two parts of the acid: it will ſink the thermometer from that temperature nearly to 0, and the conſequence of more ſalt being required is, its retaining the cold rather longer. This mixture has one great recommendation, a ſaving of time and trouble. A little water in a phial, immerſed in a ſmall tea-cup of this mixture, will be ſoon frozen in ſummer; and if the ſalt be added in cryſtals unpounded to double aqua fortis, even at a warm temperature, the cold produced will be ſufficient to freeze water or creams; but if diluted with one-fifth its weight of water, and cooled, it is about equal to the diluted nitrous acid above mentioned, and requires the ſame proportion of the ſalt. A mixture of vitriolated natron and diluted nitrous acid ſunk the thermometer from + 70° (temperature of air and ingredients) to + 10°.

The cold in any of theſe mixtures may be kept up a long time by occaſional additions of the ingredients in the proportions mentioned. A chemiſt would make the ſame materials ſerve his purpoſe repeatedly.

Equal parts of muriated ammonia and nitrated kali in powder make a cheap and convenient compoſition for producing cold

cold by ſolution in water; it will, by the following management, freeze water or creams at Midſummer.

June 12th, 1787, a very hot day, I poured four ounces, wine meaſure, of pump-water, at the temperature of 50° (it is well known that water at ſprings retains nearly the ſame temperature winter and ſummer, *viz.* about 50°, to which temperature the water may be reduced during the warmeſt weather, by pumping off ſome firſt) upon three ounces, Avoirdupois weight, of the above powder (previouſly cooled by immerſing the veſſel containing it in other water at 50°), and after ſtirring the mixture its temperature was 14°; ſome water contained in a ſmall phial, immerſed in this mixture, was conſequently ſoon frozen. This ſolution was afterwards evaporated to dryneſs, in an earthen veſſel, reduced to powder, and added to the ſame quantity of water, under the ſame circumſtances as before, when it again ſunk the thermometer to 14°. Since that time I have repeatedly uſed a compoſition of this kind for the purpoſe of producing cold, without obſerving any diminution in its effect after many evaporations. The cold may be œconomically kept up and regulated any length of time, by occaſionally pouring off the clear ſaturated liquor, and adding freſh water, obſerving to ſupply it conſtantly with as much of the powder as it will diſſolve.

The degree of cold at which water begins to freeze has been obſerved to vary much; but that it might be cooled twenty-two degrees below its freezing point was perfectly unknown to me until lately. I filled the bulb of two thermometers, one with the pureſt rain-water I could procure, the other with pump water; the water was then made to boil in each, until one-third only remained: theſe were kept in a frigorific mixture, at the temperature of +10°, for a much longer time than

than I thought neceſſary to cool the water to the ſame temperature; and by repeated trials I found it was neceſſary to lower the temperature of the mixture to near +5°, to make the water in either of them freeze. Theſe were likewiſe ſuſpended out of doors, cloſe to a thermometer, during the late froſt, and the water never obſerved frozen. On March the 22d, at ſix in the morning, the water in each remained unfrozen, though the tubes were gently ſhaken, the thermometer ſtanding at that time at 23°. There appeared to be little difference with reſpect to the degree of cold neceſſary to freeze the water, whether the tube of the thermometers were open or cloſed in vacuo (which was very nearly effected by ſuffering the water to boil up to the orifice of the tube, and then ſuddenly ſealing it) or not, but unboiled water in the ſame ſituation froze in a higher temperature.

It is commonly ſuppoſed, I believe, that gentle agitation of any kind will diſpoſe water (cooled below its freezing point) to become ice; but I have repeatedly cooled rain-water and pump-water, boiled a long time, and unboiled, in open veſſels to 30° or lower, and have conſtantly ſucceeded, after trying other kinds of agitation in vain, by ſtirring, or rather ſcraping gently, the bottom and ſides of the veſſel containing the water to be frozen, when after ſome ſhort time ſmall filaments of ice appeared, and by continuing this motion about every part of the veſſel beneath the ſurface of the water, about two-thirds of the water commonly froze. A ſlender, pointed glaſs rod I uſed for this purpoſe.

I have the honour to be, &c.

RICH. WALKER.

Oxford, March 27, 1788.

Extract from a ſecond Letter from Mr. Walker *to* Henry Cavendiſh, *Eſq. Dated* Oxford, May 28, 1788.

A more intenſe cold may be produced by a ſolution of ſalts in water in ſummer, than can be produced by a mixture of ſnow and ſalt in winter. To rain-water ſix drachms (by weight) I added ſix drachms of nitrated ammonia reduced to a very fine powder which made the thermometer ſink from +50° (temperature of the materials) to 4°, then adding ſix drachms of mineral alkali very finely powdered the thermometer ſunk to −7°, fifty-ſeven degrees. It is obſervable, that in the latter there are two cauſes concur in producing the effect, the liquifaction both of the ſnow and ſalt; but in the experiment juſt mentioned the liquifaction of the ſalts only.

Vitriolated natron, after it had given out its water of cryſtallization by expoſure to the atmoſphere, produced no change of temperature by ſolution in the diluted nitrous acid, but during ſolution in water produced heat, as did likewiſe the mineral alkali.

I have ſince my laſt ſeen FAHRENHEIT's Experiments on the freezing of Water, related in Vol. XXXIII. of the Philoſophical Tranſactions; but as mine differ in degree I take no farther notice of them.

XXVI. *A Description of an Inſtrument which, by the turning of a Winch, produces the Two States of Electricity without Friction or Communication with the Earth. In a Letter from Mr.* William Nicholſon *to Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 5, 1788.

SIR,

THE following account of the inſtrument I had the honour to ſhew to yourſelf and other philoſophical friends, in the month of March laſt, will, I hope, appear ſufficiently intereſting to be communicated to the learned Society, over which you ſo worthily preſide.

Plate VI. fig. 1. repreſents the apparatus ſupported on a glaſs pillar 6¼ inches long. It conſiſts of the following parts. Two fixed plates of braſs, A and C, are ſeparately inſulated and diſpoſed in the ſame plane, ſo that a revolving plate B may paſs very near them, without touching. Each of theſe plates is two inches in diameter; and they have adjuſting pieces behind, which ſerve to place them accurately in the required poſition. D is a braſs ball, likewiſe of two inches diameter, fixed on the extremity of an axis that carries the plate B. Beſides the more eſſential purpoſe this ball is intended to anſwer, it is ſo

loaded within on one ſide, that it ſerves as a counterpoiſe to the revolving plate, and enables the axis to remain at reſt in any poſition. The other parts may be diſtinctly ſeen in fig. 2. The ſhaded parts repreſent metal and the white repreſent varniſhed glaſs. ON is a braſs axis, paſſing through the piece M, which laſt ſuſtains the plates A and C. At one extremity is the ball D already mentioned; and the other is prolonged by the addition of a glaſs ſtick, which ſuſtains the handle L and the piece GH ſeparately inſulated. E, F, are pins riſing out of the fixed plates A and C, at unequal diſtances from the axis. The croſs-piece GH, and the piece K, lie in one plane, and have their ends armed with ſmall pieces of harpſichord-wire, that they may perfectly touch the pins EF in certain points of the revolution. There is likewiſe a pin I, in the piece M, which intercept a ſmall wire proceeding from the revolving plate B.

The touching wires are ſo adjuſted, by bending, that when the revolving plate B is immediately oppoſite the fixed plate A, the croſs-piece GH connects the two fixed plates, at the ſame time that the wire and pin at I form a communication between the revolving plate and the ball. On the other hand, when the revolving plate is immediately oppoſite the fixed plate C, the ball becomes connected with this laſt plate, by the touching of the piece K againſt F; the two plates, A and B, having then no connection with any part of the apparatus. In every other poſition the three plates and the ball will be perfectly unconnected with each other.

Mr. CAVALLO's diſcovery, ſo well explained in the laſt BAKERIAN Lecture, that the minute differences of electrization in bodies, whether occaſioned by art or nature, cannot be completely deſtroyed in any definite time, may be applied to explain

explain the action of the preſent inſtrument. When the plates A and B are oppoſite each other, the two fixed plates A and C may be conſidered as one maſs; and the revolving plate B, together with the ball D, will conſtitute another maſs. All the experiments yet made concur to prove, that theſe two maſſes will not poſſeſs the ſame electric ſtate; but that, with reſpect to each other, their electricities will be plus and minus. Theſe ſtates would be ſimple and without any compenſation, if the maſſes were remote from each other; but as that is not the caſe, a part of the redundant electricity will take the form of a charge in the oppoſed plates A and B. From other experiments I find that the effect of the compenſation on plates oppoſed to each other, at the diſtance of one-fortieth part of an inch, is ſuch that they require, to produce a given intenſity, at leaſt one hundred times the quantity of electricity that would have produced it in either, ſingly and apart. The redundant electricities in the maſſes under conſideration will therefore be unequally diſtributed: the plate A will have about ninety-nine parts, and the plate C one; and, for the ſame reaſon, the revolving plate B will have ninety nine parts of the oppoſite electricity, and the ball D one. The rotation, by deſtroying the contacts, preſerves this unequal diſtribution, and carries B from A to C, at the ſame time that the tail K connects the ball with the plate C. In this ſituation, the electricity in B acts upon that in C, and produces the contrary ſtate, by virtue of the communication between C and the ball; which laſt muſt therefore acquire an electricity of the ſame kind with that of the revolving plate. But the rotation again deſtroys the contact, and reſtores B to its firſt ſituation oppoſite A. Here, if we attend to the effect of the whole

revolution, we ſhall find that the electric ſtates of the reſpective maſſes have been greatly increaſed: for the ninety-nine parts in A and in B remain, and the one part of electricity in C has been increaſed ſo as nearly to compenſate ninety-nine parts of the oppoſite electricity in the revolving plate B, while the communication produced an equal mutation in the electricity of the ball. A ſecond rotation will, of courſe, produce a proportional augmentation of theſe increaſed quantities; and a continuance of turning will ſoon bring the intenſities to their maximum, which is limited by an exploſion between the plates.

If one of the parts be connected with an electrometer, more eſpecially that of BENNET, theſe effects will be very clearly ſeen. The ſpark is uſually produced by a number of turns between eleven and twenty; and the electrometer is ſenſibly acted upon by ſtill fewer. When one of the parts is occaſionally connected with the earth, or when the adjuſtment of the plates is altered, there are ſome variations in the effects, not difficult to be reduced to the general principles, but ſufficiently curious to excite the meditations of perſons the moſt experienced in this branch of natural philoſophy. An attention to brevity, however, renders it neceſſary to forbear enlarging upon them.

If the ball be connected with the lower part of BENNET's electrometer, and the plate A with the upper part, and any weak electricity be communicated to the electrometer, while the poſition of the apparatus is ſuch that the croſs-piece GH touches the two pins; a very few turns will render it perceptible. But here, as well as in the common doubler, the effect is rendered uncertain by the condition, that the communicated electricity muſt be ſtrong enough to deſtroy and predominate

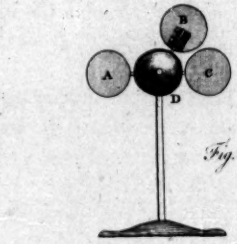


Fig. 1.

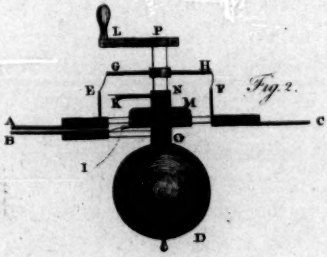


Fig. 2.

over any other electricity the plates may poſſeſs. I ſcarcely need obſerve, that if this difficulty ſhould hereafter be removed, the inſtrument will have great advantages as a multiplier of electricity in the facility of its uſe, the very ſpeedy manner of its operation, and the unequivocal nature of its reſults.

I have the honour to be, &c.

W. NICHOLSON.

XXVII. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain at* Lyndon *in* Rutland; *with the Rain in* Hampſhire *and* Surrey, *in* 1787. *Alſo ſome Account of the Annual Growth of Trees. By* Thomas Barker, *Eſq. Communicated by* Thomas White, *Eſq. F.R.S.*

Read June 12, 1788.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.				Hampſhire.		Surrey.
		Higheſt	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean	Lyndon	Sel-bourn.	Fyfield.	S. Lambeth.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	30,13	29,11	29,70	45	34½	39	47	25	35	0,415	0,88	0,43	0,60
	Aftern.				46	34½	40	51	31	39½				
Feb.	Morn	29,85	28,15	29,40	47	41	44	47½	27	39	0,860	3,67	3,40	1,68
	Aftern				48½	41½	45	53½	37	46				
Mar.	Morn.	30,02	28,47	29,30	49	41	45½	49	31	40	1,782	4,28	3,80	1,62
	Aftern.				50½	42	46½	54	35½	48½				
Apr.	Morn.	30,00	28,54	29,47	50	44	46½	50½	35½	42	1,721	0,74	0,69	0,93
	Aftern.				50½	45	47½	56½	42½	50				
May	Morn.	29,86	28,80	29,47	60½	42½	52½	55½	36	48½	1,573	2,06	1,27	1,60
	Aftern.				62	44	54	72½	46½	59				
June	Morn.	29,76	29,05	29,44	63	51½	58	60½	46	54½	1,800	1,50	1,43	0,68
	Aftern.				64½	53	59½	77	55	65½				
July	Morn.	29,93	28,92	29,36	69	56½	60½	66	50½	57	3,169	6,53	3,50	4,12
	Aftern.				70	57	62	79	63	68				
Aug.	Morn.	29,94	28,74	29,53	70	55½	61	63½	49½	56	1,969	0,83	0,74	0,60
	Aftern.				73	56½	63	80	51½	67½				
Sept.	Morn.	30,01	28,59	29,50	60	53½	57	51½	42	51	1,225	1,56	1,47	0,78
	Aftern.				60½	55	57½	65	55	61				
Oct.	Morn.	29,70	28,48	29,26	56	46½	51½	55	36	46	3,726	5,04	3,44	2,41
	Aftern.				57	47	53	61	44½	54½				
Nov.	Morn.	29,95	28,50	29,33	52	34	43	49	20	35½	1,462	4,09	2,57	1,51
	Aftern.				52½	35½	43	54½	30½	42				
Dec.	Morn.	29,93	28,75	29,22	48	34	41	48½	26	37	3,085	5,06	3, 8	3,87
	Aftern.				49	35	41½	55½	30½	41				
											Inches 22,787	36,24	25,82	20,40

The

The year began dry, with but little and moderate froſt, and proved a pleaſant and open winter: as February approached it grew mild, was fine, forward, and drying, and continued forward all March, but more ſhowery the beginning and end of that month. With April the weather turned colder; eaſt and north winds the firſt three weeks ſtopped vegetation, and after a ſhowery latter end of the month. May had ſo much cloudy weather and cold winds as blaſted the apples, and hindered the growth of graſs; but being dry was not unpleaſant, and ſuited the grain very well, which proved good.

The bees were remarkably forward in drones this year, which they had the middle of April; but the cold winds and cloudy weather afterward ſo hindered their working, that they killed their drones in May, and were backwarder the beginning of June than they had been ſeveral weeks before.

There were ſeveral froſty mornings in May, and one on the 7th of June, ſo ſharp as to kill the young ſhoots on ſome of the oaks and aſh trees in the meadows. The ſeaſon was in general dry the firſt half of the year, and the crops of hay were light. It was a very cool ſummer; what hot weather there was came toward the end of June, the beginning of July, and beginning of Auguſt.

July 6, a very ſhowery ſeaſon began, which continued with but few intermiſſions to the end of the year, and was more remarkable for the frequency than the quantity of rain, which was not very great here. This made great plenty of graſs and turneps; but was very troubleſome in hay-time and harveſt, yet more hindering than real hurting. The wetteſt months were July, October, and December. The chief fair fits were a hot week in Auguſt, and a clear froſty week at the

full moon in November. The latter part of the ſummer was very cool, the autumn mild, and not very froſty; but ſome great ſtorms, and in the countries where they had more rain than we, there were great floods. At the full moon in December there came a week's froſt and a large ſnow, which was not quite gone at the end of the year.

On the annual growth of trees.

Oaks.

		Girth. In.		Girth. In.	Rate. In.		Girth. In.	Rate. In.
1	——	——	1772	19	—	1787	41	1.5
2	1758	13	1772	33	1.4	1787	55½	1.5
3	1758	18½	1772	40½	1.6	1787	58	1.2
4	——	——	——	——	—	1787	156½	1.3
5	1758	41	1772	56	1.1	1787	77½	1.4
6	1744	20	1765	45	1.2	1787	74½	1.3
7	1758	18	1772	36	1.3	1787	54	1.2
8	1758	76	1772	93½	1.25	1787	109½	1.1
9	1751	124	1772	147	1.1	1787	164½	1.2
10	1744	23½	1765	49	1.2	1787	74	1.1
11	1744	69½	1772	99	1.2	1787	115	1.1
12	1744	14	1765	43	1.4	1787	60	0.8
13	1747	82	1765	99½	1.0	1787	120½	1.0
14	1744	21	1765	45	1.1	1787	64	0.9
15	1762	106½	1772	117	1.0	1787	130	0.9
16	1751	117	1770	132	0.8	1787	149½	1.0
17	1751	114	1770	131½	0.9	1787	145	0.8
18	1751	84½	1772	101	0.8	1787	109	0.5
19	1744	41	1765	58½	0.8	1787	69	0.5

Aſh.

Ash.

		Girth.		Girth.	Rate.		Girth.	Rate.
		In.		In.	In.		In.	In.
20	——	——	1772	71	—	1787	106	2.3
21	1745	23½	1765	67	2.2	1787	111	2.0
22	1744	22	1765	55½	1.6	1787	92	1.7
23	1744	32	1765	61	1.4	1787	94½	1.5
24	1744	66	1765	91½	1.4	1787	114	1.0
25	1751	20	1772	45	1.25	1782	58½	0.9
26	1765	55	1772	64	1.2	1787	75½	0.9
27	1747	77	1765	97	1.0	1787	116½	1.0
28	——	——	1772	67½	—	1787	82	1.0
29	1744	56	1765	77½	1.0	——	——	——
30	1755	51½	1772	67	0.9	1787	80	0.9
31	——	——	1765	74	—	1781	89	0.9
32	1751	45½	1772	67	1.0	1787	77	0.7
33	1744	17½	1765	34	0.8	1787	52	0.8
34	1744	17	1765	36½	0.9	1787	52½	0.7
35	1744	26	1772	40	0.7	——	——	——
36	1745	13½	1772	31½	0.7	1787	41	0.6

Elms.

37	1755	0	1772	42	2.5	1787	77	2.3
38	1744	28	1765	60	1.5	1787	96	1.6
39	1744	37	1758	50	0.9	1781	72	1.0
40	1744	46	1758	58	0.9			
41	1744	48	1758	59	0.8			

As I do not remember to have seen any particular account of the annual growth of trees, I here send an abstract of some observations I have made on a considerable number, for a good many years back; and have distinguished in the table the different kinds, and placed the most growing first.

Except the two firſt aſh trees, I find the growth of oak and aſh to be nearly the ſame. I have ſome of both ſorts planted at the ſame time, and in the ſame hedges, of which the oaks are the largeſt, but there is no certain rule as to that. The common growth of an oak or an aſh is about an inch in girth in a year; ſome thriving ones will grow an inch and an half; the unthriving ones not ſo much, ſome probably leſs than any here, for I choſe in general to meaſure thoſe that ſeemed thriving.

Great trees grow more timber in a year than ſmall ones; for if the annual growth be an inch, a coat of one-ſixth of an inch is laid on all round, and the timber added to the body every year is its length multiplied into the thickneſs of the coat, and into the girth, and therefore the thicker the tree is, the more timber is added. The body of N° 9 is 9 feet long, the girth under the bark above 13 feet, the thickneſs of the coat $\frac{1}{6}$ of an inch or $\frac{1}{72}$ of a foot: then $9 \times 13 \times \frac{1}{72}$ is one foot and ſix-tenths of timber added in a year to the body, beſide the increaſe on all the branches, and it has a very great head; one limb ſquares 20 inches, and is itſelf equal to a moderate tree.

The hedge in which N° 4. grows was planted in 1665, probably the tree is not older than that year; it has therefore increaſed in girth about 1.3 inch every year ſince it was ſet.

The oak, N° 5. I believe ſowed itſelf; and I did not know there was ſuch an one till about the year 1740, when the hedge being cut, the tree was found, and might be then 20 years old or more.

The two aſh trees N° 20. and 21. grow much faſter than any of the reſt, but are neither of them handſome growing trees. N° 20. has ſeveral ſeams where the bark is parting from the

the wood, and are likely to be dead ſides. N° 21. was about as thick as a walking-ſtick in 1730. It does not grow round and ſmooth, has no dead ſide, but ſeveral deep furrows in it, ſo that theſe two trees ſeem to grow faſter than they can grow well.

In 1733, N° 23, was about as thick as a pitch-fork ſhaft.

The elm, N° 37. was planted with the quick in January, 1756, and cut down to the ground as that was. It is a kind of witch elm, which grow faſter than the upright ones, and with great round heads. N° 38. is ſo far like a witch elm, that at ten feet high it parts into a great head; but it grows much ſtraighter and handſomer than that kind of tree generally does.

Planted trees at a diſtance from the hedge ſeem not to grow ſo large as ſown trees in the hedge: whether from the check the roots receive in tranſplanting, or that the trees not in hedges are more rubbed by the cattle; perhaps both cauſes concur when the trees are tranſplanted large; but trees ſet in quicks, when very ſmall, do not ſeem to be hurt by it. I have ſome oaks ſet with the quick, and a row of acorns was ſome years after ſown againſt it; but in between forty and fifty years they have not overtaken the planted ones in ſize; the ſown ſeem however inclined to be taller trees than the planted.

XXVIII. *On the Era of the Mahometans, called the Hejerà* * (هجرة). *By* William Marſden, *Eſq. F. R. S. and A. S.*

Read June 12, 1788.

IN their computation of time, the Arabs, and other Mahometan nations, reckon by a year which is purely lunar. It has no reference to the ſolar revolutions, and is of courſe unconnected with the viciſſitude of ſeaſons. The purpoſe of its adoption appears to have been chiefly religious, for the regulation of faſts and ceremonies, rather than of the civil concerns of the people. Perhaps a conſcious ignorance in matters of ſcience might have determined the inſtitutors to prefer a period whoſe limits were marked and obvious to the ſenſes, to one whoſe ſuperior accuracy depended upon aſtronomical calculation; and it may alſo be conjectured, that their habits of life rendered the adjuſtment of the tropical year leſs intereſting to theſe turbulent and wandering fanatics, than to nations whoſe attention was directed to agriculture and other peaceful arts.

The era of the Mahometans, called by them the Hejerà, or Departure, is accounted from the year of the flight of Maho-

* As this mode of ſpelling the word differs from that commonly followed, it may be proper to obſerve, that the Arabic letters of which it is compoſed are *H, j, r, à* or *ah*, and that the ſupplied vowels are to be pronounced ſhort.

met, their prophet, from Mecca, in Arabia Petræa, to Medina, at that time called Yatreb, which was the thirteenth of his pretended miſſion, the year of Chriſt 622, and of the Julian period 5335. This event, but little memorable in itſelf, and deriving no celebrity from the circumſtances immediately attending it, was, eighteen years after, diſtinguiſhed by the Caliph OMAR, as the criſis of their new religion, and eſtabliſhed as an epoch, to which the dates of all the tranſactions of the faithful ſhould have reference in future (*a*) *. Previous to this, the people had been accuſtomed to compute from the commencement of a particular war, the day of a remarkable battle, or other occaſional event of importance to their little communities (*b*). Accordingly, MAHOMET is ſaid to have been born in the firſt year of the era of the *Elephant*, ſo called from an attack on the city and temple of Mecca, by a king of Abyſſinian race, in which thoſe animals were employed; and twenty years after this, the *impious* war, in which the animoſity of two contending tribes occaſioned them to violate the ſacred or interdicted months, appeared of conſequence ſufficient to give riſe to a new era. The uncertainty and confuſion produced by this fluctuation demanded a reform, and more forcibly in proportion as the intereſts and concerns of the growing empire extended themſelves. A diſpute between two individuals, reſpecting the year in which the term of an obligation for money ſhould be underſtood to expire (the parties being agreed as to the month), pointed out to the Caliph, to whoſe tribunal it was referred, the immediate neceſſity of enjoining the obſervance of a determinate era, in which the ſtrongeſt prejudices of the people ſhould be made to

* See the authorities at the end, under this and the ſubſequent letters.

concur

concur with the ſovereign authority. The date of the Hejerà was thenceforth expreſſed in all the public acts and letters.

It muſt be underſtood, that although the account of the years, collectively conſidered, was vague, that of the months was certain, and their ſucceſſion at all times ſcrupulouſly attended to. OMAR did not think it expedient to attempt any innovation as to the time of beginning the year, againſt which the ideas of the people would have revolted; and therefore, although the eſcape of MAHOMET from the indignation of his fellow-citizens was effected, according to their records, on the firſt day of the third month, or Rabee prior (on the twelfth day of which he reached Medina), yet the Hejerà takes date from a period two months antecedent to this flight, namely, from the firſt day of Moharram, being the day on which immemorial cuſtom had eſtabliſhed the celebration of the feſtival of the new year (*c*).

The Arabian and Syrian Chriſtians, and the Mahometan aſtronomers in general, appear to have fixed this day to Thurſday the fifteenth of the Syro-Macedonian month Tamooz, anſwering to our July; but ſome among the latter, and moſt of their hiſtorical writers, refer it to the next day, Friday the ſixteenth, and this latter date has, in modern times, obtained almoſt univerſal acceptance (*d*). A religious preference which Friday claims above the reſt of the week, ſeems to have given effect to the arguments in its favour (*e*). The difference of opinion on this ſubject has ariſen, in the firſt place, from the uncertainty unavoidably attending a date, to be aſcertained, at a diſtant period of time, from the phaſe of the moon, which is retarded or advanced by ſo complicated a variety of circumſtances (*f*): and the ambiguity appears, in the ſecond place, to have been promoted by the cuſtom of the

Arabs

Arabs beginning their day at ſun-ſet; conformably with which idea, the time when the moon became viſible at Mecca, being the evening of Thurſday the fifteenth, according to our mode of computation *, was to them the commencement of Friday; which Friday (beginning a few hours later) we term the ſixteenth of July. At that period the cycle of the ſun was 15; the cycle of the moon, or golden number, 15; the Roman indiction 10; and the dominical letter C.

The year of the Mahometans conſiſts of twelve lunar months, and no emboliſm being employed to adjuſt it to the ſolar period (as practiſed by the Chaldæans and Hebrews, who were in other particulars their guides, and anciently, it is ſaid, by the Arabs themſelves), the commencement of each ſucceſſive lunar year anticipates the completion of the ſolar, and revolves through all its ſeaſons, the months reſpectively preſerving no correſpondence (*g*).

In order to form a juſt and accurate idea of the length of this year, and of its component months, it will be neceſſary to diſtinguiſh two modes of eſtimating their commencement and duration. Theſe, though their difference is not progreſſive (never amounting to more than two whole days, and rarely to ſo much as one), may yet, if miſunderſtood, occaſion, in ſome inſtances, uncertainty and error: and more eſpecially as the writers on this ſubject have inadvertently fallen

* The new moon happened in July 622, on the 14th day, at 5½ hours, A.M. Greenwich time, or about 8 hours Mecca time; and at ſun-ſet of the ſame day, the moon was 5¼ degrees before the ſun in longitude, and in 40 minutes ſouth latitude, and therefore about 4¾ degrees above the horizon. On the 15th, at ſun-ſet, it was 18 ¼ before the ſun in longitude, in 37′ north latitude, and about 15°¼ above the horizon, conſequently viſible with clear weather. The ſun ſets at Mecca, on the 15th July, at 6 h. 40′, and the twilight is there conſiderably ſhorter than in the high latitudes.

into

into contradictions, from neglecting to explain to their readers a diſtinction of which they muſt have been themſelves ſufficiently aware *. Theſe modes may be denominated the vulgar or practical, and the political or chronological reckoning.

The vulgar or practical reckoning is that which eſtimates the commencement of the year, or firſt day of the month Moharram, from the appearance of the new moon, on the evening of the firſt or ſecond day after the conjunction, or from that time at which it might from its age be viſible, if not obſcured by the circumſtances of the weather, which is ſcarcely ever ſo ſoon as twenty-four hours, and ſeldom later than forty-eight hours, after the actual change. This appearance is announced by perſons placed on the pinnacles of the moſques or other elevated ſituations, to the people below, who welcome it with the ſound of inſtruments, firing of guns, and other demonſtrations of reſpect and zeal †. The month thus commenced is computed to laſt till the new moon again becomes viſible; and ſo of the remaining months, till ſhe has completed her twelfth lunation, and, emerging from the ſun's rays, marks the practical commencement of another year (*b*).

In the political or chronological mode of reckoning, the return of a new year, or the duration of the months which compoſe it, is not regulated either by the appearance of the moon, or the calculated period of conjunction, but according to a certain diviſion of a cycle of thirty years, adopted for this

* The juſtice of this remark will appear evident by contraſting the authorities quoted under the letters (*b*) (*i*) and (*m*).

† Theſe ſalutations are more ſolemn or clamourous at the return of ſome months than of others, and particularly on the appearance which terminates the month of faſting, or Ramadan.

purpoſe.

purpose*. Particular attention is due to the explanation of this mode, both as being more artificial and complex, and because it serves to regulate the dates in matters of historical record, and indeed of all writings where pretension is made to accuracy (*k*). Upon this the Turkish, Moorish, and every systematic Mahometan calendar are founded.

The lunar month, or mean synodic revolution, according to the computation of the Arabian astronomers, consists of 29 days, 12 hours, and 792 scruples or parts in 1080; and the year of 354 days, 8 hours, and 864 scruples. But, as the purposes of mankind require that the year should contain an integral number of days, it became expedient to collect and dispose of these fractional exceedings in a consistent and practicable manner; and with this view, a cycle or period of thirty lunar years was chosen, as the lowest number that admitted of their being formed into days, without sensible deficiency or remainder. Their sum being 11 days, it was determined that 19 of those thirty years should be composed of 354 days, and 11 of 355 days each. The justness of this proportion will equally appear, if it be observed, that 8 hours and 864 scruples (or 48 minutes) constitute 11 parts in 30 of twenty-four hours, and consequently in thirty years produce an excess of 11 whole days†. It remained next to be considered

* A passage in ALFRAGANUS (who wrote about the year of Christ 950) would lead us to infer, that, beside the two ways of computing time here distinguished, the Astronomers were accustomed to follow a third, whose periods were marked by the conjunction of the luminaries: but, as this learned Mahometan was a professed student of PTOLEMY's works (which in this place he quotes), we may conclude, that, when he speaks of astronomers, he does not mean to confine the expression to those of his own country or religion (*i*).

† The mean synodic revolution being 29 d. 12 h. 44′ and nearly 3″, this cycle

dered in what order and method theſe additional or intercalary days ſhould be inſerted, ſo as to effect the compenſation required with as much equability as poſſible, and maintain a correſpondence, as near as circumſtances would admit, with the periods marked by the phaſes of the moon. The following are the years to which, for reaſons that ſhall be afterwards aſſigned, it was judged proper to annex an extraordinary day, and which (in contradiſtinction to thoſe 19 that have only 354 days) are termed *years of exceſs*, viz. the 2d, 5th, 7th, 10th, 13th, 16th, 18th, 21ſt, 24th, 26th, and 29th, of the cycle of thirty years.

Their months, conformably with thoſe of the Hebrew calendar, it was determined ſhould conſiſt alternately of 30 and 29 days; and therefore, in an ordinary or ſimple year of 354 days, the twelfth and laſt month, Dulhajee, would have only 29; but, in the years of exceſs, the intercalary day is added to this month, which is then made to conſiſt of 30 days, and the year, conſequently, of 355 days (*m*). Thus, for example, in the year of Chriſt 622, the Hejerà commenced on the 16th of July, with the Arabian month

	Days.		Days.
Moharram, which had	30	Moharram, in the 2d year, had	30
Safar - - -	29	Safar - - -	29
Rabee prior - -	30	Rabee prior - -	30
Carried over	89	Carried over	89

falls ſhort of thirty complete lunar years, by ſomething more than 17, and conſequently advances one day in about 2500 years. The Chaldæans, who made the time of the revolution to conſiſt of one ſcruple, or 1080th part of an hour, more than the Arabs thought fit to allow, were wonderfully near to the truth (*l*).

If, inſtead of thirty years, a cycle of nineteen had been choſen, and ſeven days intercalated, there would have been an exceſs of a thirtieth part of a day, which would have cauſed the reckoning to retrograde one day in 570 years.

Rabee

	Days.
Brought over	89
Rabee poſterior -	29
Joomad prior - -	30
Joomad poſterior -	29
Rajab - - -	30
Saban - - -	29
Ramadan - -	30
Sawal - - -	29
Dulkaidat - -	30
Dulhajee - -	29
Days	354

Ended 5 July 623.

	Days.
Brought over	89
Rabee poſterior - -	29
Joomad prior - -	30
Joomad poſterior -	29
Rajab - - -	30
Saban - - -	29
Ramadan - - -	30
Sawal - - -	29
Dulkaidat - - -	30
Dulhajee - - -	30
Days	355

Ended 25 June 624.

It may not be unintereſting to examine the rule by which the Arabians appear to have been guided, in placing the intercalary day at the end of thoſe particular years which have been ſpecified. It was obſerved, that the annual exceſs is calculated to be 11 parts in 30 of a day. At the commencement of the firſt year of their firſt cycle, they appear to have aſſumed the fact (ſomewhat capriciouſly) that there was then an exceſs of 11 parts, belonging to the preceding year, to be accounted for, or brought on. At the end of the firſt year there would conſequently be 22 ſuch parts; and at the end of the ſecond year 33 parts. Here then the firſt intercalary day was applied; that ſecond year was made to conſiſt of 355 days, and there remained 3 parts, over and above, to be carried on to the next.

At the expiration of the third year, the parts amounted to 14; of the fourth year, to 25; and of the fifth, to 36; when the intercalation was again applied, and a balance of 6 parts carried on. From this it will be underſtood in what manner the fractional exceedings of each year were combined and diſpoſed of through the ſucceeding years of the cycle; and it will be neceſſary only further to remark, that, when the aggregate of the fractions falls ſhort no more than 2 or 3 parts of the number of 30, they ſtill add the intercalary day, and deduct the deficiency from the exceſs of the following year, which, in the courſe of one cycle, takes place only three times. At the end of the 29th year, the accumulated fractions, amounting exactly to 30, are commenſurate with the intercalation then applied; and the exceſs of the 30th, or laſt year, is accounted for in the firſt intercalation of the ſucceeding period. The operation would doubtleſs have appeared more methodical, if the firſt intercalary day were not to have been added till the end of the third year, and the eleventh, or laſt, till the end of the 30th year or termination of the cycle. From this conſideration ſome commentators have been led to diſſent from the more general idea, as above given, and to ſuggeſt, that the emboliſm is in fact applied ſo ſoon after the commencement of the cycle, as the yearly accumulation of the fractional parts exceeds the ſum of half a day, or twelve hours, and that it accordingly is made to take place at the end of the ſecond year, becauſe the fractions then amount to 17 h. 36′, or 22 parts in 30; at the end of the fifth year, becauſe they then amount to 25; and at the end of the ſeventh year, to 17 parts; keeping thus as near as poſſible to the mean diviſion of time, by applying the compenſation before it is fully wanted. The effect, however, is in both caſes the ſame,

and

and it is of but little moment to determine which theory is right *.

This cycle of thirty Mahometan years, contains 10,631 days, and is equal to 29 years and 39 days of our computation. The annual mean difference is 10 days and 21 hours nearly; which in common calculations, for ſhort periods of time, may be reckoned at 11 days, by which number the lunar year anticipates the ſolar.

Annexed hereto is a table exhibiting the correſpondence of the years of the Hejerà, from the eſtabliſhment of that epoch, with thoſe of the Chriſtian era, to the year of our Lord 2000. Until the beginning of the preſent century, it appears ſufficient to diſtinguiſh every tenth year; the intervals between which may be calculated with eaſe and preciſion, by attending to what has been ſaid reſpecting the cycle. From the year 1700 to the concluſion of the twentieth century, for the convenience of hiſtorians yet unborn, the commencement of each year of the Hejerà is aſcertained. Theſe tables are founded upon thoſe of GRAVIUS (J. GREAVES), in his Epochæ celebriores ULUG BEIGI, publiſhed in 1650; but as he, in conformity with the principles of this celebrated Tartarian aſtronomer †, has fixed the epoch of the Hejerà to the 15th July,

* The former hypotheſis is ſupported by CHRISTMANNUS (Commentarius ad caput primum ALFRAGANI, 1590), who quotes many ancient authorities. PETAVIUS (PETEAU) (de Doctrinà Temporum, 1627) is inclined to give the preference to the latter, on the authority of PAULUS FOROSEMPRONIUS, and the probability of the matter (*w*). It does not appear at what time the uſe of this cycle was introduced, but probably ſubſequent to the eſtabliſhment of the era by OMAR, though he is ſaid to have been aſſiſted on that occaſion by a learned Perſian.

† ULUG BEIG was the grandſon of TIMUR the Great (TAMERLANE), to whoſe empire he ſucceeded on the death of his father SHAH RUKH. He was born in 1393, and died in 1449.

inſtead

inſtead of the 16th, or hiſtorical period, it was judged requiſite to add one day, throughout, to his calculations. The propriety of this alteration is ſtrengthened by the authority of other chronologiſts *, and by the practice of the modern almanacs †. It is alſo to be obſerved, that the tables of GRAVIUS, having been compoſed in the ſeventeenth century, are calculated both for paſt and future time, according to the old ſtyle; and as the change took place, in England, in September of the year 1752, it was neceſſary to adjuſt all the ſucceeding years to the new calendar. In order that a judgment may be formed of the correſpondence of the annual periods ſhewn by theſe tables, founded on the cycle of thirty years, which is adjuſted to the mean motion of the moon, with thoſe marked by the appearance of that planet, a ſhort table is ſubjoined, containing a compariſon of the reſults of the two modes of reckoning, during one cycle, commencing with the year of the Hejerà 1171 (*p*).

* See Tables of the Hejerà in RICCIOLI, Chronologia Reformata, 1659. Ephemerides MAT. FRED. BECKII, 1695 (*o*).

† According to the original tables of GREAVES, the firſt day of Moharram, in the year of CHRIST 1783, falls on the 14th November, O.S., or 25th November, N.S.; and in 1784, on the 2d November, O.S., or 13th November, N.S.; whereas, by two almanacs, printed at Calcutta in Bengal, it appears, that the days ſhould be the 26th and 14th November. Of theſe almanacs, the one was compiled in the "Office of the Miſſion;" and the other by an ingenious aſtronomer from England; and both founded on the uſage of the Mahometans of India.

AUTHORITIES.

(*a*) "Epochæ verò hujus usum introduxit OMAR, qui primus titulo hoc literas et diplomata signari jussit; et quidem anno ejusdem epochæ demum 18, mense Gjumadâ posteriore; ut ex Historiâ Sarac Ibn Amîdi, et aliunde liquet." GOLIUS, notæ in ALFERGANUM, 1669.

(*b*) "Antè illud tempus similis quoque Arabibus mos fuerat putare suos annos à Bellorum initiis, et celebrioribus pugnis." GOLIUS ut suprà.

(*c*) "Arabes annos Higræ seu migrationis Muhammedicæ numerant à neomeniâ Muharram; licet Muhammed Meccâ excesserit die primo Rabiæ prioris, diebus 59 post epochæ ipsius diem." GOLIUS ut suprà.

(*d*) "Initium hujus epochæ est principium Moharram, illius anni in quo Propheta noster MOHAMMADES à Meccâ ad Medinam migrabat; et illud secundùm medium calculum, est feria quinta (dies Jovis), sed secundùm phasim Lunæ, dies Veneris." ULUG BEIG, 1449, per GRAVIUM, 1650.

"Æra Arabum ducitur à principio ejus anni, quo Muhammed, relictâ Meccâ, commigravit Medinam: eratque istius anni initium feria quinta." ALFERGANUS, 950, per GOLIUM, 1669.

"Sequimur autem Saracenum, cùm facimus initium annorum Hegiræ à feriâ sextâ: is enim ita scribit: 'Quidam incipiunt computationem mensium à nocte quintæ feriæ, et ponunt ibi Almuharam anni repulsionis: et quidam ponunt ipsum à nocte diei Veneris, et secundùm illud fixus est iste noster liber: quoniam iste dies fuit initium anni veri, et est magis conveniens ortui novæ lunæ.' Ex quibus verbis perspicere licet,

nos Hegiram incipere à phasi lunari, quæ incidit in principium feriæ sextæ, et minimùm integro die post conjunctionem mediam solis et lunæ in oculos incurrit." CHRISTMANNUS, 1590.

(*e*) "At sextus peculiariter dies Conventûs dicitur; quia eo, utpote sibi sacra, in templum cathedrale convenire solent." GOLIUS ut suprà.

"Commodùm igitur tunc evenit, ut in eundem diem feria sexta, quæ Veneri consecrata fuit, et neomenia popularis, ac luna corniculata, &c." PETAVIUS de Doctrina Temporum.

(*f*) "Causæ autem primam Lunæ visionem vel retardantes vel promoventes, tres potissimùm in astronomiâ redduntur. Prima est obliquitas sphæræ, longos vel breves occasus adducens: in signis namque longarum descensionum, nempe in Piscibus, Ariete, et Tauro, fieri potest, ut prima phasis Lunæ paulò post conjunctionem conspiciatur. Altera causa est tempus conjunctionis Lunæ cum sole circa limitem Boreum. Tertia denique, si luna sit motu velox, qualis est circa Perigæum." HEVELII Selenographia, 1647.

(*g*) "Ita fit, ut primus illorum mensis, qui est Muharram, nullam certam in anno Juliano sedem habeat; sed quotannis antevertat; ac totum anni nostri contextum peragret." PETAVIUS, Rationarium Temporum, 1702.

(*h*) "Mahomedani menses hujus epochæ à phasi novilunii ad phasim sequentis novilunii numerant. E duodecim mensibus annum constituunt. Anni, itaque, et menses, secundùm eorum usum, sunt lunares veri." ULUG BEIG.

"Voluerant autem Arabes à vesperâ diem auspicari; quoniam à phasi lunari incipiunt numerare dies mensium." ALFRAGANUS, per CHRISTMANNUM.

" Omnes illæ gentes, quæ anno lunari ſunt uſæ, non ſolum hanc primam Lunæ apparitionem diligenter attenderunt, ſed etiam numerum principiumque cujuſlibet menſis ab eâ inierunt." HEVELIUS.

(*i*) " Menſes, ſecundùm PTOLEMÆUM, computantur ab unâ mediâ ſolis et lunæ conjunctione, uſque ad alteram: verus autem menſis incipit poſt digreſſum luminarium, elapſo uno die naturali." ALFRAGANUS, per CHRISTMANNUM.

(*k*) " Neque enim tempus quod à phaſi petitur, certò deſignari poteſt, hominibus præſertim longiùs diſſitis, aut poſt venturis; quod res civiles ac politicæ requirunt: aſtronomicæ verò et chronologicæ conſiſtere abſque eo neutiquam poſſunt. Sacræ, autem, quia juxta phaſin primùm inſtitutæ fuerant, juxta eandem quoque obſervari debuerunt." GOLIUS ut ſupra.

(*l*) " Satis intelligitur, voluiſſe Arabes de induſtriâ recedere à calendario Judaico, commodioremque annorum ſuorum periodem et rationem intercalandi obſervare." CHRISTMANNUS.

(*m*) " Aſtronomis, Moharram (menſis primus) triginta eſt dierum, et Safar (ſecundus) undetriginta, et ſimiliter menſium unus triginta dierum, et alius undetriginta uſque ad finem anni. Singulis autem annis tricenis, menſem Dulheggiah (poſtremum) undecies conſtituunt triginta dierum; atque hoc fit anno ſecundo, et quinto, &c." ULUG BEIG.

" Sunt autem menſes quidam 30, nonnulli 29 dierum; ut annus ſimplex conſtet 354 diebus. Menſes Arabum in phaſi lunari variant pro luminis additione vel diminutione: ideo rectè conſtitutum eſt, ut menſes alternatim ſint pleni et cavi. Spacio triginta annorum accreſcunt undecim dies intercalares: ratione veræ anni quantitatis, quæ ponitur 354 dierum et $\frac{11}{30}$ diei. Annus, in quo fractiones colliguntur, habet 7 menſes plenos et 5 cavos. Menſis cui adhibetur intercalatio ſemper eſt dierum 30; unde annus intercalaris nuncupatur, qui menſi

Dhilhaga (Dulhajee) diem adjicit, ut notum eſt omnibus." ALFRAGANUS, per CHRISTMANNUM.

(*n*) "Sed civili ratione tum dies unus intercalatur, cùm horariæ appendices dimidiatum diem ſuperant, ut Paulus exiſtimat. Alii ut CHRISTMANNUS, &c." PETAVIUS, Doctrina Temporum.

(*o*) "G. KIRCHIUS in calendario Chriſt°-Jud°-Turc°, præfati anni 1687 uno die ſerius dictum annum Turcarum recenſet." MAT. FRID. BECKIUS, Eph. 1695.

(*p*) "Non enim ſemper contingit, initium menſis, ratione, et calculi et primæ phaſeos, incidere in eundem diem: neque hoc fit, niſi tractu temporis ambo inter ſe æquentur." ALFERGANUS, per GOLIUM.

Table exhibiting the correſpondence of the years of the Hejerà with thoſe of the Chriſtian era.

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1	622	16 July	F	181	797	5 Mar.	Su	361	971	24 Oct.	Tu
11	632	29 Mar.	Su	191	806	17 Nov.	Tu	371	981	7 July	Th
21	641	10 Dec.	M	201	816	30 July	W	381	991	20 Mar.	F
31	651	24 Aug.	W	211	826	13 Apr.	F	391	1000	1 Dec.	Su
41	661	7 May	F	221	835	26 Dec.	Su	401	1010	15 Aug.	Tu
51	671	18 Jan.	Sa	231	845	7 Sept.	M	411	1020	27 Apr.	W
61	680	1 Oct.	M	241	855	22 May	W	421	1030	9 Jan.	F
71	690	15 June	W	251	865	2 Feb.	F	431	1039	23 Sept.	Su
81	700	26 Feb.	Th	261	874	16 Oct.	Sa	441	1049	5 June	M
91	709	9 Nov.	Sa	271	884	29 June	M	451	1059	17 Feb.	W
101	719	24 July	M	281	894	13 Mar.	W	461	1068	31 Oct.	F
111	729	5 Apr.	Tu	291	903	24 Nov.	Th	471	1078	14 July	Sa
121	738	18 Dec.	Th	301	913	7 Aug.	Sa	481	1088	27 Mar.	M
131	748	31 Aug.	Sa	311	923	21 Apr.	M	491	1097	9 Dec.	W
141	758	14 May	Su	321	933	1 Jan.	Tu	501	1107	22 Aug.	Th
151	768	26 Jan.	Tu	331	942	15 Sept.	Th	511	1117	5 May	Sa
161	777	9 Oct.	Th	341	952	29 May	Sa	521	1127	17 Jan.	M
171	787	22 June	F	351	962	9 Feb.	Su	531	1136	29 Sept.	Tu

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
541	1146	13 June	Th	971	1563	21 Aug.	Sa	1139	1726	18 Aug.	Th
551	1156	25 Feb.	Sa	981	1573	3 May	Su	1140	1727	8 Aug.	Tu
561	1165	7 Nov.	Su	991	1583	15 Jan.	Tu	1141	1728	27 July	Sa
571	1175	22 July	Tu	1001	1592	28 Sept.	Th	1142	1729	16 July	W
581	1185	4 Apr.	Th	1011	1602	11 June	F	1143	1730	6 July	M
591	1194	16 Dec.	F	1021	1612	23 Feb.	Su	1144	1731	25 June	F
601	1204	29 Aug.	Su	1031	1621	6 Nov.	Tu	1145	1732	13 June	Tu
611	1214	13 May	Tu	1041	1631	20 July	W	1146	1733	3 June	Su
621	1224	24 Jan.	W	1051	1641	2 Apr.	F	1147	1734	23 May	Th
631	1233	7 Oct.	F	1061	1650	15 Dec.	Su	1148	1735	13 May	Tu
641	1243	21 June	Su	1071	1660	27 Aug.	M	1149	1736	1 May	Sa
651	1253	3 Mar.	M	1081	1670	11 May	W	1150	1737	20 Apr.	W
661	1262	15 Nov.	W	1091	1680	23 Jan.	F	1151	1738	10 Apr.	M
671	1272	29 July	F	1101	1689	5 Oct.	Sa	1152	1739	30 Mar.	F
681	1282	11 Apr.	Sa	1111	1699	19 June	M	1153	1740	18 Mar.	Tu
691	1291	24 Dec.	M					1154	1741	8 Mar.	Su
701	1301	6 Sept.	W	1112	1700	7 June	F	1155	1742	25 Feb.	Th
711	1311	20 May	Th	1113	1701	28 May	W	1156	1743	15 Feb.	Tu
721	1321	31 Jan.	Sa	1114	1702	17 May	Su	1157	1744	4 Feb.	Sa
731	1330	15 Oct.	M	1115	1703	6 May	Th	1158	1745	23 Jan.	W
741	1340	27 June	Tu	1116	1704	25 Apr.	Tu	1159	1746	13 Jan.	M
751	1350	11 Mar.	Th	1117	1705	14 Apr.	Sa	1160	1747	2 Jan.	F
761	1359	23 Nov.	Sa	1118	1706	4 Apr.	Th	1161	1747	22 Dec.	Tu
771	1369	5 Aug.	Su	1119	1707	24 Mar.	M	1162	1748	11 Dec.	Su
781	1379	19 Apr.	Tu	1120	1708	12 Mar.	F	1163	1749	30 Nov.	Th
791	1388	31 Dec.	Th	1121	1709	2 Mar.	W	1164	1750	19 Nov.	M
801	1398	13 Sept.	F	1122	1710	19 Feb.	Su	1165	1751	9 Nov.	Sa
811	1408	27 May	Su	1123	1711	8 Feb.	Th	1166	1752	8 Nov.	W
821	1418	8 Feb.	Tu	1124	1712	29 Jan.	Tu	1167	1753	29 Oct.	M
831	1427	22 Oct.	W	1125	1713	17 Jan.	Sa	1168	1754	18 Oct.	F
841	1437	5 July	F	1126	1714	7 Jan.	Th	1169	1755	7 Oct.	Tu
851	1447	19 Mar.	Su	1127	1714	27 Dec.	M	1170	1756	26 Sept.	Su
861	1456	3 Nov.	M	1128	1715	16 Dec.	F	1171	1757	15 Sept.	Th
871	1466	13 Aug.	W	1129	1716	5 Dec.	W	1172	1758	4 Sept.	M
881	1476	26 Apr.	F	1130	1717	24 Nov.	Su	1173	1759	25 Aug.	Sa
891	1486	7 Jan.	Sa	1131	1718	13 Nov.	Th	1174	1760	13 Aug.	W
901	1495	21 Sept.	M	1132	1719	3 Nov.	Tu	1175	1761	2 Aug.	Su
911	1505	4 June	W	1133	1720	22 Oct.	Sa	1176	1762	23 July	F
921	1515	15 Feb.	Th	1134	1721	11 Oct.	W	1177	1763	12 July	Tu
931	1524	29 Oct.	Sa	1135	1722	1 Oct.	M	1178	1764	1 July	Su
941	1534	13 July	M	1136	1723	20 Sept.	F	1179	1765	20 June	Th
951	1544	25 Mar.	Tu	1137	1724	9 Sept.	W	1180	1766	9 June	M
961	1553	7 Dec.	Th	1138	1725	29 Aug.	Su	1181	1767	30 May	Sa

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1182	1768	18 May	W	1225	1810	5 Feb.	Tu	1268	1851	26 Oct.	M
1183	1769	7 May	Su	1226	1811	25 Jan	Sa	1269	1852	14 Oct.	F
1184	1770	27 Apr	F	1227	1812	15 Jan.	Th	1270	1853	3 Oct.	Tu
1185	1771	16 Apr.	Tu	1228	1813	3 Jan	M	1271	1854	23 Sept.	Su
1186	1772	5 Apr.	Su	1229	1813	23 Dec.	F	1272	1855	12 Sept.	Th
1187	1773	25 Mar.	Th	1230	1814	13 Dec.	W	1273	1856	31 Aug.	M
1188	1774	14 Mar.	M	1231	1815	2 Dec.	Su	1274	1857	21 Aug.	Sa
1189	1775	4 Mar.	Sa	1232	1816	20 Nov.	Th	1275	1858	10 Aug.	W
1190	1776	21 Feb.	W	1233	1817	10 Nov.	Tu	1276	1859	31 July	M
1191	1777	9 Feb.	M	1234	1818	30 Oct.	Sa	1277	1860	19 July	F
1192	1778	30 Jan.	F	1235	1819	19 Oct.	W	1278	1861	8 July	Tu
1193	1779	19 Jan.	Tu	1236	1820	8 Oct.	M	1279	1862	28 June	Su
1194	1780	8 Jan.	Sa	1237	1821	27 Sept.	F	1280	1863	17 June	Th
1195	1780	28 Dec.	Th	1238	1822	17 Sept.	W	1281	1864	5 June	M
1196	1781	17 Dec.	M	1239	1823	6 Sept.	Su	1282	1865	26 May	Sa
1197	1782	7 Dec.	Sa	1240	1824	25 Aug.	Th	1283	1866	15 May	W
1198	1783	26 Nov.	W	1241	1825	15 Aug.	Tu	1284	1867	4 May	Su
1199	1784	14 Nov.	Su	1242	1826	4 Aug.	Sa	1285	1868	23 Apr.	F
1200	1785	4 Nov.	F	1243	1827	24 July	W	1286	1869	12 Apr.	Tu
1201	1786	24 Oct.	Tu	1244	1828	13 July	M	1287	1870	2 Apr.	Su
1202	1787	13 Oct.	Sa	1245	1829	2 July	F	1288	1871	22 Mar.	Th
1203	1788	2 Oct.	Th	1246	1830	22 June	W	1289	1872	10 Mar.	M
1204	1789	21 Sept.	M	1247	1831	11 June	Su	1290	1873	28 Feb.	Sa
1205	1790	10 Sept.	F	1248	1832	30 May	Th	1291	1874	17 Feb.	W
1206	1791	31 Aug.	W	1249	1833	20 May	Tu	1292	1875	6 Feb.	Su
1207	1792	19 Aug.	Su	1250	1834	9 May	Sa	1293	1876	27 Jan.	F
1208	1793	9 Aug.	F	1251	1835	28 Apr.	W	1294	1877	15 Jan.	Tu
1209	1794	29 July	Tu	1252	1836	17 Apr.	M	1295	1878	4 Jan.	Sa
1210	1795	18 July	Sa	1253	1837	6 Apr.	F	1296	1878	25 Dec.	Th
1211	1796	7 July	Th	1254	1838	26 Mar.	Tu	1297	1879	14 Dec.	M
1212	1797	26 June	M	1255	1839	16 Mar.	Su	1298	1880	3 Dec.	Sa
1213	1798	15 June	F	1256	1840	4 Mar.	Th	1299	1881	22 Nov.	W
1214	1799	5 June	W	1257	1841	22 Feb	Tu	1300	1882	11 Nov.	Su
1215	1800	24 May	Su	1258	1842	11 Feb.	Sa	1301	1883	1 Nov.	F
1216	1801	14 May	F	1259	1843	31 Jan.	W	1302	1884	20 Oct.	Tu
1217	1802	3 May	Tu	1260	1844	21 Jan.	M	1303	1885	9 Oct.	Sa
1218	1803	22 Apr.	M	1261	1845	9 Jan.	F	1304	1886	29 Sept.	Th
1219	1804	11 Apr.	Th	1262	1845	29 Dec.	Tu	1305	1887	18 Sept.	M
1220	1805	31 Mar.	M	1263	1846	19 Dec.	Su	1306	1888	7 Sept.	Sa
1221	1806	20 Mar.	F	1264	1847	8 Dec.	Th	1307	1889	27 Aug.	W
1222	1807	10 Mar.	W	1265	1848	26 Nov.	M	1308	1890	16 Aug.	Su
1223	1808	27 Feb.	Su	1266	1849	16 Nov.	Sa	1309	1891	6 Aug.	F
1224	1809	15 Feb.	Th	1267	1850	5 Nov.	W	1310	1892	25 July	Tu

An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.	An. Hej.	An. D.		Day.
1311	1893	14 July	Sa	1348	1929	7 June	Su	1385	1965	30 Apr.	Su
1312	1894	4 July	Th	1349	1930	27 May	Th	1386	1966	20 Apr.	F
1313	1895	23 June	M	1350	1931	17 May	Tu	1387	1967	9 Apr.	Tu
1314	1896	11 June	F	1351	1932	5 May	Sa	1388	1968	29 Mar.	Su
1315	1897	1 June	W	1352	1933	24 Apr	W	1389	1969	18 Mar.	Th
1316	1898	21 May	Su	1353	1934	14 Apr.	M	1390	1970	7 Mar.	M
1317	1899	11 May	F	1354	1935	3 Apr.	F	1391	1971	25 Feb.	Sa
1318	1900	29 Apr.	Tu	1355	1936	22 Mar.	Tu	1392	1972	14 Feb.	W
1319	1901	18 Apr.	Sa	1356	1937	12 Mar.	Su	1393	1973	2 Feb.	Su
1320	1902	8 Apr.	Th	1357	1938	1 Mar.	Th	1394	1974	23 Jan.	F
1321	1903	28 Mar.	M	1358	1939	19 Feb	Tu	1395	1975	12 Jan.	Tu
1322	1904	16 Mar.	F	1359	1940	8 Feb.	Sa	1396	1976	2 Jan.	Su
1323	1905	6 Mar.	W	1360	1941	27 Jan.	W	1397	1976	21 Dec.	Th
1324	1906	23 Feb.	Su	1361	1942	17 Jan.	M	1398	1977	10 Dec.	M
1325	1907	12 Feb.	Th	1362	1943	6 Jan.	F	1399	1978	30 Nov.	Sa
1326	1908	2 Feb.	Tu	1363	1943	26 Dec.	Tu	1400	1979	19 Nov.	W
1327	1909	21 Jan.	Sa	1364	1944	15 Dec.	Su	1401	1980	7 Nov.	Su
1328	1910	11 Jan.	Th	1365	1945	4 Dec.	Th	1402	1981	28 Oct.	F
1329	1910	31 Dec.	M	1366	1946	24 Nov.	Tu	1403	1982	17 Oct.	Tu
1330	1911	20 Dec.	F	1367	1947	13 Nov.	Sa	1404	1983	6 Oct.	Sa
1331	1912	9 Dec.	W	1368	1948	1 Nov.	W	1405	1984	25 Sept.	Th
1332	1913	28 Nov.	Su	1369	1949	22 Oct.	M	1406	1985	14 Sept.	M
1333	1914	17 Nov.	Th	1370	1950	11 Oct.	F	1407	1986	4 Sept.	Sa
1334	1915	7 Nov.	Tu	1371	1951	30 Sept.	Tu	1408	1987	24 Aug.	W
1335	1916	26 Oct.	Sa	1372	1952	19 Sept.	Su	1409	1988	12 Aug.	Su
1336	1917	16 Oct.	Th	1373	1953	8 Sept.	Th	1410	1989	2 Aug.	F
1337	1918	5 Oct.	M	1374	1954	28 Aug.	M	1411	1990	22 July	Tu
1338	1919	24 Sept.	F	1375	1955	18 Aug.	Sa	1412	1991	11 July	Sa
1339	1920	13 Sept.	W	1376	1956	6 Aug.	W	1413	1992	30 June	Th
1340	1921	2 Sept.	Su	1377	1957	27 July	M	1414	1993	19 June	M
1341	1922	22 Aug.	Th	1378	1958	16 July	F	1415	1994	8 June	F
1342	1923	12 Aug.	Tu	1379	1959	5 July	Tu	1416	1995	29 May	W
1343	1924	31 July	Sa	1380	1960	24 June	Su	1417	1996	17 May	Su
1344	1925	20 July	W	1381	1961	13 June	Th	1418	1997	7 May	F
1345	1926	10 July	M	1382	1962	2 June	M	1419	1998	26 Apr.	Tu
1346	1927	29 June	F	1383	1963	23 May	Sa	1420	1999	15 Apr.	Sa
1347	1928	18 June	W	1384	1964	11 May	W	1421	2000	4 Apr.	Th

Com-

Comparison of the commencement of thirty successive years, according to the division of the cycle, and the appearance of the new moon.

	A. H.	A. C.	Cycle.	Conjunction. Day.	Hour.	☽ visible.
1	1171	1757	15 Sept.	13 Sept.	14	15 Sept.
2	—1172	1758	4 Sept.	2 Sept.	15	4 Sept.
3	1173	1759	25 Aug.	22 Aug.	16	24 Aug.
4	1174	—1760	13 Aug.	11 Aug.	1	12 Aug.
5	—1175	1761	2 Aug.	31 July	15	2 Aug.
6	1176	1762	23 July	21 July	9	23 July
7	—1177	1763	12 July	10 July	23	12 July
8	1178	—1764	1 July	29 June	8	30 June
9	1179	1765	20 June	18 June	11	20 June
10	—1180	1766	9 June	7 June	12	9 June
11	1181	1767	30 May	27 May	19	29 May
12	1182	—1768	18 May	16 May	8	17 May
13	—1183	1769	7 May	6 May	1	7 May
14	1184	1770	27 April	25 April	17	27 April
15	—1185	1771	16 April	15 April	3	16 April
16	1186	—1772	5 April	3 April	5	4 April
17	1187	1773	25 Mar.	23 Mar.	5	24 Mar.
18	—1188	1774	14 Mar.	12 Mar.	10	14 Mar.
19	1189	1775	4 Mar.	1 Mar.	22	3 Mar.
20	1190	—1776	21 Feb.	19 Feb.	13	21 Feb.
21	—1191	1777	9 Feb.	8 Feb.	5	9 Feb.
22	1192	1778	30 Jan.	28 Jan.	15	30 Jan.
23	1193	1779	19 Jan.	17 Jan.	18	19 Jan.
24	—1194	—1780	8 Jan.	6 Jan.	17	8 Jan.
25	1195	1780	28 Dec.	25 Dec.	20	27 Dec.
26	—1196	1781	17 Dec.	15 Dec.	6	16 Dec.
27	1197	1782	7 Dec.	4 Dec.	21	6 Dec.
28	1198	1783	26 Nov.	24 Nov.	13	26 Nov.
29	1199	—1784	14 Nov.	13 Nov.	1	14 Nov.
30	1200	1785	4 Nov.	2 Nov.	4	3 Nov.

The years marked thus — are the intercalated and bissextile years in the Mahometan and Roman calendars.

PRESENTS

MADE TO THE

ROYAL SOCIETY

From November 1787 to June 1788;

WITH

The NAMES of the DONORS.

	Preſents.	Donors.
1787.		
Nov. 8.	Archæologia. Vol. VIII. London, 1787. 4°	The Society of Antiquaries.
	An Account of the ancient Painting at Cowdray, repreſenting the Proceſſion of K. Edward VI. from the Tower of London to Weſtminſter. 4°	———
	A Print from the ſaid Picture.	———
	Librorum impreſſorum qui in Muſeo Britannico adſervantur Catalogus. 2 Vol. Londini, 1787. fol.	The Truſtees of the Britiſh Muſeum.
	Bibliothecæ Bodleianæ codicum Manuſcriptorum Orientalium Catalogus, a Jo. Uri confectus. Pars I. Oxonii, 1787. fol.	The Truſtees of the Clarendon Preſs, Oxford.
	Tranſactions of the Society for Encouragement of Arts, Manufactures, and Commerce. Vol. V. London, 1787. 8°	The Society for Encouragement of Arts, Manufactures, and Commerce.
	Rapport des Commiſſaires chargés des Projets relatifs à l'Etabliſſement des quatre Hôpitaux. Paris, 1787. 4°	The Royal Academy of Sciences of Paris.

Nov.

1787.	Presents.	Donors.
Nov. 8.	Arrêt du Conseil d'Etat sur l'Etablissement de quatre nouveaux Hopitaux, du 22 Juin, 1787. 4°	The Royal Academy of Sciences of Paris.
	Hydraulic and Nautical Observations on the Currents in the Atlantic Ocean, by Gov. Pownall. London, 1787. 4°. With a Chart of the Atlantic Ocean.	Thomas Pownall, Esq. F.R.S.
	A botanical Arrangement of British Plants, by W. Withering. Birmingham, 1787. Vol. I. and II. 8°	William Withering, M. D. F. R. S.
	Observations de M. Trebra sur l'interieur des Montagnes, avec un Discours preliminaire, et des Notes, par M. le Baron de Dietrich. Paris, 1787. fol.	Baron de Dietrich.
	Nic. Jos. Jacquin Collectanea Botanicam, Chemiam et Historiam Naturalem spectantia. Vol. I. Vindobonæ, 1786. 4°	Professor de Jacquin, of Vienna, F. R. S.
	Kort over det Sydlige Norge, ved C. I. Pontoppidan, 1785.	Professor Bugge, of Copenhagen, F. R. S.
	Kort over den Sydlige deel af Fyen tilligemed det tilgrændsende stykke af Hertugdommet Schlesvig, &c. tegnet af H. Shanke, aar 1783.	
	The History and Antiquities of Aston Flamvile and Burbach, by John Nichols. London, 1787. 4°	Mr. John Nichols.
	The Heetopades of Veeshnoo-Sarma, in a Series of connected Fables, translated from the Sanskreet Language, by C. Wilkins. Bath, 1787. 8°	Charles Wilkins, Esq. F.R.S.
	A Map of the County of Clare in Ireland, by H. Pelham, in 12 Sheets.	Mr. Henry Pelham.
	Medical Commentaries for the Year 1786, collected by A. Duncan. Edinburgh, 1787. 8°	Andrew Duncan, M. D.
	Exposition raisonnée de la Theorie de l'Electricité et du Magnetisme. Paris, 1787. 8°	Abbé Haüy.
	Quarta Dissertatio Botanica de Gerania, Auctore A. J. Cavanilles. Parisiis, 1787. 4°	Abbé Cavanilles.

1787.	Presents.	Donors.
Nov. 8.	Principes sur l'Art des Accouchemens, par J. L. Baudelocque. Paris, 1787. 12°	M. Baudelocque.
	Essai sur l'Education intellectuelle, avec le Projet d'une nouvelle Science, par A. C. Chavannes. Lausanne, 1787. 8°	Professor Chavannes, of Lausanne.
	Astronomisches Jahrbuch für das Jahr 1789, von J. E. Bode. Berlin, 1786. 8°	M. J. E. Bode, Astronomer to the Academy of Berlin.
	J. J. Hemmers Anleitung Wetterleiter an allen gattungen von gebäuden anzulegen. Manheim, 1786. 8°	M. Hemmer.
	Entdeckungen über die Theorie des Klanges, von E. F. F. Chladni. Leipzig, 1787. 4°	Dr. Chladni, of Wittenberg.
	Metrophanis Critopuli Emendationes in J. Meursii Glossarium Græcobarbarum. Edidit J. G. F. Franzius. Stendaliæ, 1787. 8°	Professor Franz, of Leipsig.
	A Synopsis of a Course of Lectures on the Theory and Practice of Medicine. Part I. Boston, 1786. 8°	Professor Waterhouse, of Cambridge in America.
15.	Rachunku Algebraicznego teorya przystósowaná do Linii krzywych. 2 Vols. Krakow, 1783. 4°	Professor Sniadecki.
22.	Carte generale de l'Ocean Atlantique, dressée au depôt general des Cartes, Plans, et Journaux de la Marine, 1786.	Marquis de Chabert, F.R.S.
	Analyse de la Carte generale de l'Ocean Atlantique. Paris, 1786. 4°	———
	Extraits des Observations Astronomiques et Physiques faites à l'Observatoire Royal, en l'Année 1786, par M. le Comte de Cassini, Directeur. 4°	Comte de Cassini.
	A Map of the Moon.	———
	Commentarii de Rebus in Scientia Naturali et Medicina gestis. Volumen XXVIII. Lipsiæ, 1786. 8°	Mr. Hurlock, F.R.S.
	Memoirs of William Lambarde, Esq. 4°	Mr. John Nichols.
	La Vie de l'Homme respectée et defendue dans ses derniers Momens. Paris, 1787. 8°	M. Thiery.

1787.	Presents.	Donors.
Nov. 22.	Eſſay on the Cauſes of the Variety of Complexion and Figure in the Human Species, by Sam. Stanhope Smith. Philadelphia, 1787. 8°	The Rev. Dr. Williams, Secretary to the American Academy.
Dec. 6.	Elements of Conic Sections, by C. Hutton. London, 1787. 8°	Charles Hutton, LL.D.F.R.S.
	Inſtitutiones Phyſiologicæ, Auctore L. M. A. Caldanio. Venetiis, 1786. 8°	Profeſſor Caldani, of Padua, F. R. S.
	Inſtitutiones Pathologicæ, Auctore L. M. A. Caldanio. Venetiis, 1786. 8°	
	Nova Analyſeos Elementa, Auctore J. B. Nicolai. Tom. I. Pars I. Patavii, 1786. 4°	Profeſſor Nicolai, of Padua.
	Riforma de' Carri di quattro Ruote, del Conte L. Rizzetti. Trevigi, 1785. 8°	Conte Luigi Rizzetti.
	Relacion de las Epidemias de Calenturas putridas y malignas, en el Principado de Cataluna, por Don J. Maſdevall. Madrid, 1786. 4°	Don Joſeph Maſdevall.
	Relation abregée d'un Voyage à la Cime du Mont-Blanc, en Août 1787, par H. B. de Sauſſure. Geneve. 8°	Fowler Walker, Eſq.
13.	Leçons de Calcul differentiel et de Calcul integral, par M. Couſin. 2 Vols. Paris, 1777. 8°	M. Couſin.
20.	Appendix to Dr. Garthſhore's Obſervations on numerous Births, printed in the Philoſophical Tranſactions, Vol. LXXVII. 4°	Maxwell Garthſhore, M. D. F. R. S.
	Meteorological Diary, kept at Madras, from Sept. 14, 1786, to Feb. 17, 1787. MS. fol.	William Petrie, Eſq.
	An Account of Experiments made by Mr. Miller in the Firth of Forth, the 2d of June, 1787, in a double Veſſel. MS. fol.	Patrick Miller, Eſq.
	MS. Papers of Zachary Williams, on making Sea Water freſh, the Longitude, &c.	Mr. John Nichols.
	A Print of John Hunter, Eſq. F. R. S. engraved by W. Sharp.	Mr. William Sharp.
	A Print of Sir William Hamilton, K.B. F. R. S. engraved by H. Hudſon.	Mr. Henry Hudſon.

	Preſents.	Donors.
1787.		
Dec. 20.	A Print of John Henniker, Eſq. F.R.S. engraved by the ſame.	Mr. Henry Hudſon.
1788.		
Jan. 10.	Mémoires de l'Academie Royale des Sciences. Années 1770, 1772. Partie I. 1779, 1780, 1781, et 1782.	Royal Academy of Sciences of Paris.
	Mémoires preſentées à l'Academie Royale des Sciences, par divers Savans. Tomes VII. IX. et X.	———
	Table generale des Matieres contenues dans les Mémoires de l'Academie Royale des Sciences, Tomes VII. et VIII.	———
	Vari componimenti della Accademia degli Etnei per la morte di Ignazio Vincenzo Paterno Caſtello Principe V. di Biſcari. Catania, 1787. 8°	The Prince of Biſcari.
	Elogio d' Ignazio Principe di Biſcari, di Giov. Ardizzone. Catania, 1787. 8°	———
	Funebris laudatio Egnatii Biſcarorum Principis, a Raym. Platania. Catinæ, 1787. 8°	———
	Orazione funebre d' Ignazio Principe di Biſcari. Catania, 1787. 8°	———
	Elogio d' Ignazio Principe di Biſcari, di Domen. Privitera. Catania, 1787. 8°	———
	Liber Sinicus Tao Te Kim inſcriptus, in Latinum Idioma verſus. MS. fol.	Matthew Raper, Eſq. F.R.S.
	Remarks on the Means of obviating the fatal Effects of the Bite of a mad Dog, by R. Hamilton. Ipſwich, 1785. 8°	R. Hamilton, M. D.
	Practical Obſervations on Venereal Complaints. Edinburgh, 1788. 8°	Francis Swediaur, M. D.
17.	The London Medical Journal. Vol. VIII. London, 1787. 8°	Samuel Foart Simmons, M. D. F. R. S.
	Obſervations on extra-uterine Caſes, by M. Garthſhore. (Part of the foregoing Medical Journal). 8°	Maxwell Garthſhore, M. D. F. R. S.
	Obſervations ſur les Effets des Vapeurs Méphitiques dans l'Homme, &c. Paris, 1787. 8°	M. Portal.

1788.	Presents.	Donors.
Jan. 17.	Avis sur les moyens pratiqués avec succès pour secourir les Personnes noyées, &c. Paris, 1787. 4°	M. Portal.
	The same, printed in fol. par.	———
24.	Medical Commentaries, for the Year 1787, collected by A. Duncan. Edinburgh, 1788. 8°	Andrew Duncan, M. D.
	A Meteorological Journal of the Year 1787, kept in Pater-noster-Row, London, by W. Bent. 4°	Mr. William Bent.
31.	Kongl. Wetenskaps Academiens nya Handlingar. Tom. VII. for 1786, 3d and 4th Quarters; and Tom. VIII. for 1787, 1st and 2d Quarters. Stockholm. 8°	The Royal Academy of Sciences of Stockholm.
	Morsels of Criticism. London, 1788. 4°	Edward King, Esq. F. R. S.
	Lettere di Fisica sperimentale di D. Ser. Serrati. Firenze, 1787. 12°	D. Serafino Serrati.
	La Neoellenopedia di Fr. Mazzarella-Farao. Parte I. e II. Napoli, 1779. 8°	Sig. Francesco Mazzarella-Farao.
	Disamina del parere di M. Voltaire sulla Poesia Epica. Opera del S. Paoli Rolli, tradotta dall' Inglese. Napoli, 1779. 8°	———
	Di Museo gli amorosi avvenimenti tra Ero e Leandro, tradotti dal Greco in Latino, ed in versi Italiani da Franc. Mazzarella-Farao, Napoli, 1787. 8°	———
Feb. 7.	Two Impressions, one coloured and one uncoloured, of a Portrait of William Herschel, LL.D. F. R. S. painted by Abbott, and engraved by Ryder.	Mr. S. Watts.
	Ja E. Smith, Disputatio inauguralis quædam de Generatione complectens. Lugd. Bat. 1786. 8°	James Edward Smith, M. D. F. R. S.
21.	Mémoires Academiques, ou nouvelles Decouvertes sur la Lumiere. Paris, 1788. 8°	M. Marat.
	Chemical Observations on Sugar, by E. Rigby. London, 1788. 8°	Mr. Edward Rigby.

	Preſents.	Donors.
1788.		
Feb. 28,	Pharmacopœia Collegii Regalis Medicorum Londinenſis. Londini, 1788. 4°	The College of Phyſicians.
	Theoretiſch-praktiſche beurtheilung des Scharbocks. Peterſburg, 1787. 8°	Dominicus Spedicati, M. D.
	Plan et Profil d'un Batiment pour conſerver et purifier les eaux à boire, propoſé par Dominique Spedicati, MS.	———
	Obſervations ſur l'Arc-en-ciel, par M. l'Abbé P. Paris, 1788. 8°	An unknown Hand.
Mar. 6.	The Deſcription and Uſe of Nairne's Patent Electrical Machine. London, 1787. 8°	Mr. Edward Nairne, F. R. S.
	Tarantiſmo obſervado en Eſpana, ſu Autor Don F. X. Cid. 1787. 4°	Don Franciſco Xavier Cid.
	F. X. Cid Diſſertatio circa inventionem Pulſus antidiſcroti tamquam veri ſigni Diarrhoeam ventoſam demonſtrantis. Toleti. 4°	———
	Fenomeno raro del primero que ſe ha conocido entrar en el Hoſpital General de Madrid por haber ſido picato del Inſecto llamado Tarantula. 4°	———
	Deſcripcion Hiſtorica de una nueva eſpecie de Corea, ó Baile de San Vito, por D. B. Pinera y Siles. Madrid, 1787. 4°	Don Bartolomé Pinera y Siles.
April 3.	Tables of the apparent Places of the Comet of 1661, whoſe return is expected in 1789. London, 1788. 4°	Sir Henry Englefield, Bart. F. R. S.
	Experiments and Obſervations on Animal Heat, and the Inflammation of combuſtible Bodies. 2d Edition. London, 1788. 8°	Adair Crawford, M.D.F.R.S.
	The Works of the late William Stark, M. D. London, 1788. 4°	James Carmichael Smyth, M. D. F. R. S.
	Deſcription de Pyrmont, traduite de l'Allemand de M. Marcard. Tome II. Leipzig, 1785. 8°	Dr. Marcard.
	A ſhort Deſcription of Pyrmont, abridged from the German of Dr. Marcard. London, 1788. 8°	———

1788.	Preſents.	Donors.
April 3.	Phyſique nouvelle, par M. Delairas. Paris, 1788. 8°	M. Delairas.
	Method to diſcover the Difference of the Earth's Diameters. London, 1788. 8°	Thomas Williams.
10.	Experiments on the Congelation of Vitriolic and Nitrous Acids, made at Fort Albany, in Hudſon's Bay, by J. M^c Nab. MS. fol.	John M^c Nab, Eſq.
	Meteorological Journal kept at Henley-houſe and at Albany Fort, from Nov. 8, 1784 to Aug. 31, 1787, by J. M^c Nab. MS. fol.	———
	Meteorological Journal kept at Henley-houſe, Hudſon's Bay, from June 3. to Aug. 20, 1787. MS. fol.	———
	Table of the mean Heights and extreme Differences of the Thermometer and Barometer at Madras, from October, 1786, to October, 1787. MS.	William Petrie, Eſq.
17.	Letters written in Holland in the Months of September and October, 1787, by T. Bowdler. London, 1788. 8°	Thomas Bowdler, Eſq. F.R.S.
	Antonii Scarpa Anatomicarum Annotationum Liber II. Ticini, 1785. 4°	Profeſſor Scarpa, of Pavia.
	Ejuſdem Oratio de promovendis Anatomicarum Adminiſtrationum rationibus, 1783. 4°	———
	Metodo per coſtruire e dirigere le Macchine Aeroſtatiche, di F. Henzion. Firenze, 1788. 4°	Sig. Franceſco Henzion.
24.	A Diſſertation on the Properties of Pus, by E. Home. London, 1788. 4°	Mr. Everard Home, F.R.S.
May 7.	Obſervations on the Diſeaſes of the Army in Jamaica, by J. Hunter, M. D. London, 1788. 8°	John Hunter, M. D. F. R. S.
8.	Hiſtoire de l'Academie Royale des Sciences, Année 1785. Paris, 1788. 4°	The Royal Academy of Sciences of Paris.
	A Map of Hindooſtan, by J. Rennell. In 4 Sheets.	James Rennell, Eſq. F. R. S.
	Memoir of a Map of Hindooſtan, by J. Rennell. London, 1788. 4°	———

1788.	Preſents.	Donors.
May 8.	Carte Topographique de Spa, et de ſes Environs, par T. J. Collins.	John Aſh, M. D. F. R. S.
	Eſſays Medical, Philoſophical, and Experimental; by T. Percival. 4th Edit. Vol. I. Warrington, 1788. 8°	Thomas Percival, M.D. F.R.S.
22.	A Treatiſe on Medical and Pharmaceutical Chymiſtry and the Materia Medica. 3 Vols. London, 1788. 8°	Donald Monro, M. D. F. R. S.
	An Eſſay towards a Syſtem of Mineralogy, by A. F. Cronſtedt, enlarged by J. H. de Magellan. 2 Vols. London, 1788. 8°	Mr. de Magellan, F. R. S.
	Aſtronomiſches Jahrbuch für das Jahr 1790, von J. E. Bode. Berlin, 1787. 8°	Mr. J. E. Bode, Aſtronomer to the Academy of Berlin.
	Tratado de Navegacion, por D. Joſ. de Mendoza y Rios. 2 Vols. Madrid, 1787. 4°	Don Joſef de Mendoza y Rios.
	The Aggrandizement and National Perfection of Great-Britain. 2 Vols. London. 4°	George Edwards, Eſq.
29.	An Account of the Life, Writings, and Inventions, of John Napier, of Merchiſton, by D. S. Earl of Buchan and W. Minto. Perth, 1787. 4°	David Earl of Buchan, F. R. S.
	The Hiſtory of a ſavage Girl, caught wild in the Woods of Champagne, tranſlated from the French. London. 12°	William Seward, Eſq. F. R. S.
	Eſſai analytique ſur l'Air pur, et les differentes eſpeces d'Air, par M. de la Metherie. Paris, 1788. 8°	M. de la Metherie.
	Diſſertation ſur l'effet de la Muſique dans les Maladies nerveuſes, par L. Deſbout. St. Peterſburg, 1784. 8°	M. Deſbout.
	Mémoire ſur le Scorbut, traduit de l'Allemand, de H. Bacheracht. Reval, 1787. 8°	

	Preſents.	Donors.
1788.		
May 29.	Analiſe d'un Mémoire ſur le Scorbut du Dr. Bacheracht. MS. 4°	M. Deſbout.
	Appendice du Traducteur et Redacteur de l'Extrait ſuſdit. MS. 4°	———
June 5.	Hiſtoire et Mémoires de l'Academie Royale de Sciences, Inſcriptions, et Belles Lettres de Toulouſe. Tom. I. II. et III. Toulouſe, 1782—1788. 4°	The Royal Academy of Sciences and Belles Lettres of Toulouſe.
	Troiſieme Rapport des Commiſſaires chargés par l'Academie des projets relatifs à l'etabliſſement des quatre Hôpitaux. Paris, 1788. 4°	M. Tenon.
	A Letter to George Earl of Leiceſter, from J. Henniker. London, 1788. 8°	John Henniker, Eſq. F. R. S.
	Collections for a Hiſtory of Sandwich. Part I. Canterbury, 1788.	William Boys, Eſq.
	Elemens d'Anatomie. Partie I. Paris, 1788. 4°	M. Suë le Fils.
	Nouvelle Théorie Aſtronomique, pour ſervir à la determination des Longitudes, miſe au jour, par J. Rutledge, Bart. Paris, 1788. 4°	Sir James Rutledge, Bart.
12.	A Map of the Iſland of Sicily, by R. Mylne.	Robert Mylne, Eſq. F. R. S.
	Precis d'un Ouvrage intitulé, Reflexions ſur les Hôpitaux, par M. Le Roy, MS.	M. Le Roy, F. R. S.
	Plan et Elevation d'un Projet d'Hôtel-Dieu, de M. Le Roy. 3 Plates.	———
	Connoiſſance des Tems pour l'Année 1790, publiée par M. Mechain. Paris, 1788. 8°	M. Mechain.
	Art du Potier d'etain. Part I. et II. Paris, 1788. fol.	M. Salmon.

AN INDEX TO THE SEVENTY-EIGHTH VOLUME OF THE PHILOSOPHICAL TRANSACTIONS.

A.

ACCOUNT of experiments made by Mr. John M^c Nab, at Albany Fort, Hudson's Bay, relative to the freezing of nitrous and vitriolic acids, p. 166. Account of an experiment to determine the effect of ſalt upon the expanſion of water by cold, p. 311.

Acid, vitriolic, experiments on, p. 177. Its effect upon the freezing point of water, p. 305. *Nitrous acid*, its effect on the freezing point of water, p. 306. Effect of the *Muriatic acid* in lowering the freezing point of water, p. 307.

Acidity, obſervations and experiments on the principle of, p. 147, 313.

Aerial Moiſture, devaporation of, p. 49.

Air, frigorific, experiments on the mechanical expanſion of, p. 43. Air, ſuſceptible of heat, p. 56. Fixed air contains about half its weight of water, p. 152. On the converſion of a mixture of dephlogiſticated and phlogiſticated airs into nitrous acid by the electric ſpark, p. 261. Nitrous air contains water, which contributes to the formation of fixed air, p. 318. If phlogiſticated and light inflammable air be preſented to each other at the inſtant of their ſeparation from ſolid or liquid ſubſtances, and before their particles have receded from each other, they readily combine and

generate volatile alkali, p. 381. The combination of the phlogiſticated and inflammable airs, and the formation of volatile alkali, depend chiefly, if not altogether, on the approximation of the parts of inflammable air, when phlogiſticated air is preſented to them, p. 382. Nitrous air mixed with hepatic air will form volatile alkali, p. 384.

Airs, on the affinities of the phlogiſticated and light inflammable, p. 379. See *Air*.

Albany Fort, account of experiments made there, p. 166.

Alkali, *mineral*, its effect on the freezing point of water, p. 308. *Volatile Alkali*, its effect on the ſame, p. 309. On the formation of volatile alkali, p. 379. When iron, water, and ſulphur, act upon each other in atmoſpheric air, volatile alkali is formed, p. 384. Volatile alkali formed by mixing nitrous air with hepatic air, p. 384.

America, South, account of ſome native iron found there, p. 37, 183.

Arabs and other Mahometan nations reckon their time by the lunar year, p. 414.

Atmoſphere, correſpondence of the heat of the, with the height of the barometer, p. 48.

Attraction of life, p. 29. Its cauſe can never be inveſtigated, p. 35. Study of the laws of the attraction of life, or what has been called muſcular motion, of conſiderable importance, ibid.

Auſtin, William, M. D. on the formation of volatile alkali, and on the affinities of the phlogiſticated and light inflammable airs, p. 379.

Authorities cited in Mr. Marſden's account of the era of the Mahometans, p. 425—428.

B.

Baillie, Matthew, M. D. on the tranſpoſition of the Viſcera, p. 350.

Barbary, experiment on the, p. 158.

Barker, Thomas, Eſq. Abſtract of a regiſter of the barometer, thermometer, and rain at Lyndon in Rutland; with the rain at Selbourn and Fyfield in Hampſhire, and at South Lambeth in Surrey. Alſo ſome account of the annual growth of trees, p. 408.

Barometer, ſtate of it at London throughout the year 1787, p. 192—216. Abſtract of a regiſter of the barometer, thermometer, and rain at Lyndon in Rutland, in 1787, p. 408.

Blagden, Charles, M. D. on the cooling of water below its freezing point, p. 125. On the effect of various ſubſtances in lowering the point of congelation in water, p. 277.

Borax, the effect of, on the freezing point of water, p. 293.

C.

Cactus Tuna, a kind of Indian fig, an irritable vegetable, p. 161.

Cavallo Tiberius, Obſervations on ſmall quantities of electricity, p. 1. On the temperament of thoſe muſical inſtruments in which the tones, keys, or frets are fixed, p. 238.

Deſcription

Deſcription of a new electrical inſtrument, p. 255.

Cavendiſh, Henry, Eſq. account of experiments made at Albany Fort, p. 166. On the converſion of a mixture of dephlogiſticated and phlogiſticated airs into nitrous acid, by the electric ſpark, p. 261.

Celis, Michael Rubin de, on native iron found in South America. p. 37, 183.

Centers, on moveable, p. 95.

Corollary. If the freezing point of the ſolution of a ſalt in water in any known proportion be given, its degree of ſolubility may in general be aſcertained, merely by trying the greateſt cold it will produce with ſnow, p. 296.

Corollaries, mathematical, p. 68, 70, 71, 75, 76, 81, 82, 86, 87, 90, 91, 92.

Croonian Lecture on muſcular motion, p. 23.

Cuckoo, obſervations on the natural hiſtory of, p. 219. The cuckoo makes choice of the neſts of a great variety of ſmall birds, p. 221. A cuckoo chooſes the neſt of a hedge-ſparrow to lay her egg in, p. 223. Two cuckoos and a hedge-ſparrow hatched in the ſame neſt, p. 229. Why, like other birds, the cuckoo does not build a neſt, incubate its eggs, and rear its own young, conſidered, p. 229. Subſtances found in the ſtomach of a cuckoo, p. 235.

D.

Darwin, Eraſmus, M. D. frigorific experiments. See *Air*.

Dionæa muſcipula, an irritable vegetable, p. 161. Obſervation on, p. 163.

Diſcoveries, two, made by the moderns, in the ſtructure and phyſiology of the body, viz. the circulation of the blood; and the lymphatics, and abſorption of the lymph, p. 33.

Dover Caſtle, temperature of the water in the New Well there, p. 111.

Droſera, obſervation on, p. 163.

E.

Electrometer, firſt conſtructed by Mr. John Canton, p. 3. Conical corks in electrometers preferable to pith balls, p. 5. Mr. Bennet's electrometer, p. 406.

Electricity, natural or artificial, methods of manifeſting the preſence, and aſcertaining the quality, of ſmall quantities of, p. 1. Mr. Volta's condenſer of electricity deſcribed, p. 7. Mr. Bennet's doubler of electricity deſcribed, p. 8. Not an inſtrument to be depended upon, p. 10, 14. Remarks on the ſubject of electricity in general, p. 17. Experiment ſhewing the great length of time that a quantity of electricity will remain upon a body, p. 19. The air, or in general any ſubſtance, is a more or leſs perfect conductor of electricity, according as the electricity which is to paſs through it is more or leſs condenſed, p. 20. Brief propoſal of an explanation of the production

of electricity by friction, which is dependent upon the propoſition, viz. that bodies are always in ſome degree electrified; and alſo upon the principle of the capacity of bodies for holding electric fluid being increaſed by the proximity of other bodies in certain circumſtances, p. 21. Deſcription of a new electrical inſtrument, p. 255. Deſcription of an inſtrument which produces the two ſtates of electricity without friction or communication with the earth, p. 403.

Experiment, proving that the nerves are not employed in the motions excited by ſtimuli, p. 27. Frigorific experiments on the mechanical expanſion of air, explaining the cauſe of the great degree of cold on the ſummits of high mountains, the ſudden condenſation of aerial vapour, and of the perpetual mutability of atmoſpheric heat, p. 43. Experiments on local heat, p. 103. Experiments relating to the variation of local heat in the earth itſelf, p. 110. On the Barbary, p. 158. Experiments on the cooling of Water below its freezing points, p. 125. Experiment on very hard pump-water, which generally congealed one or two degrees ſooner than unboiled diſtilled water, when placed in a frigorific mixture, p. 127. Various experiments on the cooling of water, p. 129, 130. Experiments and obſervations relating to the principle of acidity, the compoſition of water, and phlogiſton, p. 147. Additional experiments on the ſame, p. 313. Experiment on the *Rota chalappenſis*, p. 162. Experiments on ſpirit of nitre, p. 167. Experiments on ſpirit of vitriol, p. 168. Experiments on a new electrical inſtrument, p. 258, 259. Experiment on the converſion of a mixture of dephlogiſticated and phlogiſticated airs into nitrous acid, by the electric ſpark, p. 261. Experiments on the effect of various ſubſtances in lowering the point of congelation in water, p. 277. Experiment, proving that nitrous air contains water, and that this water can contribute to the formation of fixed air, p. 318. Experiments on liquor produced by burning, in a cloſe copper veſſel, inflammable air, got from Iron by means of the ſteam of water, and dephlogiſticated air, expelled by heat only, from Minium, p. 320. Experiments on liquor produced by burning, in the ſame manner, inflammable air, obtained from Iron by means of the ſteam of water, and dephlogiſticated air, expelled by heat only, from Manganeſe, ibid. Experiments on theſe liquids, made from Minium and Manganeſe, united, p. 321. Experiments on liqeor, produced by burning inflammable air, obtained as before, and dephlogiſticated air from Mercurius precipit. ruber, in a cloſe tinned iron veſſel, ibid. Experiments on the formation of volatile alkali, and on the affinities of the phlogiſticated and light inflammable airs, p. 379. Experiments on the production of artificial cold, p. 395.

F.

Fig, Indian, Cactus Tuna, a kind of, p. 161.

Fluid, electric, on the hypotheſis of a ſingle, it is ſaid, that every ſubſtance in nature, when

when not electrified, contains its proper ſhare of electric fluid, which is proportionate to its bulk, or to ſome other of its properties; and that this equal or proportionate diſtribution of electric fluid takes place with the greateſt part of natural bodies. This, however, is not the caſe; for every ſubſtance is always electrified, p. 17.

Forces, centripetal, p. 67.

Fordyce, George, M. D. on muſcular motion, p. 23.

Franklin, Dr. an obſervation of his concerning the electric fluid in glaſs, p. 121. Proved erroneous, p. 122.

G.

Georgian Planet. See *Planet*.

Glauber's Salt, its effect in lowering the freezing point of water, p. 290.

Gray, Edward Whitaker, M. D. on the manner in which glaſs is charged with the electric fluid, and diſcharged, p. 121.

H.

Hebtrden, William, M. D. a table of the mean heat of every month for ten years in London, from 1763 to 1772 inclusively, p. 66. See *Tables*.

Hedge-ſparrow, the neſt of a, choſen by a cuckoo to lay her egg in, p. 223. An inſtance of two cuckoos and a hedge-ſparrow being hatched in the ſame neſt, p. 229.

Herſchel, William, LL.D. on the Georgian Planet and its ſatellites, p. 364.

Hunter, John, M. D. on the heat of wells and ſprings in the iſland of Jamaica, and on the temperature of the earth, p. 53. See *Obſervations*.

I.

Ice of ſolutions of nitre, very different from common ice, p. 283.

Jenner, Edward, Eſq. on the natural hiſtory of the cuckoo, p. 219.

Inſtrument, deſcription of a new electrical, capable of collecting together a diffuſed or little condenſed quantity of electricity, p. 255. Deſcription of an inſtrument which, by the turning of a winch, produces the two ſtates of electricity without friction or communication with the earth, p. 403.

Inſtruments, muſical, of the temperament of, p. 238. See *Muſical Inſtruments*.

Iron, native, found in South-America, p. 37, 183.

K.

Kair, James, Eſq. letter to Joſeph Prieſtley, LL.D. p. 323. See *Letter*.

Kingſton, in Jamaica, ſupplied with water from wells, p. 58. The temperature of the air at Kingſton admits but of ſmall variation, p. 59.

L.

Letter from Dr. Withering to Joſeph Prieſtley, L L D. relrting to the principle of acidity, the decompoſition of water, and phlogiſton, p. 319. Another from James Keir, Eſq. to the ſame, on the ſame ſubject, p. 323. A letter from Richard Walker, to Henry Cavendiſh, Eſq. on the production of artificial cold, p. 395. Extract from a ſecond letter from Mr. Walker to Henry Cavendiſh Eſq. on the ſame ſubject, p. 402.
Life, attraction of, p. 29. See Attraction of life.
Lapis Naturæ, ſingular, deſcribed, p. 352.

M.

Mahometans, on the era of the, p. 414. The vulgar or practical reckoning of the Mahometans, p. 418. The political or chronological mode of reckoning among them, ibid. Upon the political or chronological mode of computation on which the Turkiſh, Mooriſh, and every ſyſtematic Mahometan calendar are founded, p. 419.
Manganeſe. See *Experiments*.
Marſden, William, Eſq. on the era of the Mahometans, called the hejerà, p. 414.
Medicago falcata, obſervations on, p. 162.
Medicine, in the hands of the Greek phyſicians from the time of Hippocrates, or rather from the deſtruction of the Egyptian monarchy by Cambyſes, down to the time of the Cruſades, p. 33.
Meteorological Journal for the year 1787 kept at the apartments of the Royal Society, p. 191. See *Tables*. Explanation of the inſtruments with which the meteorological obſervations were made, p. 217.
Mimoſa ſenſitiva and *pudica*, irritable vegetables, p. 161.
Minium. See *Experiments*.
Mixture, powerful frigorific, p. 395. Frigorific mixtures recommended, eſpecially in hot climates, p. 398, 399.
Morgan, William. See *Surviorſhips*.
Motion, all original, are by their nature perfectly unintelligible as to their cauſe, p. 35.
Mountains, coldneſs of the ſummits of, p. 47.
Muriatic acid, its effect on the freezing point of water, p. 307.
Muſcular motion, Croonian lecture on, p. 23.
Muſical Inſtruments, of the temperament of thoſe in which the tones, keys, or frets, are fixed, as in the harpſichord, organ, guittar, &c. p. 238.

N.

Native Iron, account of a mass of, found in South-America, p. 37, 183.
Nicholson, William, on an instrument which, by the turning of a winch, produces the two states of electricity without friction or communication with the earth, p. 403.
Nitre, spirit of, subject to aqueous and spirituous congelation, p. 166. The effect of nitre in lowering the freezing point of water, p. 281. The ice of solutions of nitre very different from common ice, p. 283. Nitre and common salt, the effect of a compound solution of, in lowering the freezing point, p. 299.
Nitrous acid, its effect on the freezing point of water, p. 306.
Numbers, On some properties of the sum of the divisors of, p. 388.

O.

Observations on motion in general, p. 23. On the heat of wells and springs in the island of Jamaica, and on the temperature of the earth below the surface in different climates, p. 53. Observations made at Brighthelmstone, with a view to ascertain the temperature of the sea as a bath, p. 64. On the manner in which glass is charged with the electric fluid, and discharged, p. 121. Observations and experiments relating to the principle of acidity, the composition of water, and phlogiston, p. 147, 313. Observations on the irritability of vegetables, p. 158. Observations on the natural history of the cuckoo, p. 219.

P.

Parietaria, observations on the, p. 162.
Parnassia, observation on the, p. 163.
Phlogiston, experiments and observations on, p. 147, 313.
Planet, on the Georgian, and its satellites, p. 364. The light of the satellites of this planet is, on account of their great distance, uncommonly faint, p. 376. The second satellite brighter than the first, but the difference not considerable, ibid. The situations of the orbits of these satellites do not materially differ, p. 378. They will be eclipsed about the years 1799 and 1818, ibid. Probably not less than those of Jupiter, ibid.
Platina, dust of, mixed with gold, found in the kingdom of Santa Fe de Bogotà, p. 41. 188.
Priestley, Joseph, LL.D. on the principle of acidity, the composition of water, and phlogiston, p. 147, 313.

Problems.

If the ages of two perſons be given, to determine the probabilities of ſurvivorſhip between them from any table of obſervations, p. 332.

If the ages of two perſons be given, to determine, from any table of obſervations, the preſent value of the ſum payable on the contingency of one life's ſurviving the other, p. 341.

The ages of two perſons being given, to determine the value of the ſum payable on the extinction of one life in particular, ſhould that happen after the extinction of the other life, p. 347.

Propoſitions, mathematical, p. 67, 68, 70, 72, 74, 75, 77, 84, 87, 93, 95, 96, 98.

Pure Iron, a maſs of, in the ſhape of a tree with its branches, exiſting in South-America, p. 41, 187.

Q.

Quartz, ſtones of, found in South-America, p. 41, 187.

R.

Rain at Lyndon in Rutland, at Selbourn and Fyfield in Hampſhire, and at South Lambeth in Surrey, in 1787, p. 408.

Refrigeration, a remarkable one at Glaſgow, which Mr. Wilſon perceived to take place on the ſurface of the ſnow and hoar-froſt, p. 103.

Rochelle Salt, its effect in lowering the freezing point of water, p. 289. Compound ſolution of Rochelle ſalt, common ſalt, and Sal Ammoniac, its effect in producing cold, p. 302.

Rue, common, obſervations on, p. 162, 163.

Ruta Chalepenſis, experiment on, p. 162. Obſervation on, p. 163.

S.

Sal Ammoniac, its effect in lowering the freezing point of water, p. 286. Sal cathartic amarus, its effect in producing cold, p. 291.

Sal Ammoniac and *common Salt*, their effect in lowering the freezing point of water, p. 301.

Salt, common, its effect in lowering the freezing point of water, p. 279. Salt of tartar,

its

its effect on the freezing point of water, p. 307. See *Glauber's Salt*, *Rochelle Salt*.
Santa Fe de Bogotà, duſt of platina, mixed with gold, found at, p. 41. 188.
Saxifraga, obſervation on, p. 163.
Sheerneſs, temperature of the water in the King's Well there, p. 111.
Six, James, Eſq. on local heat, p. 103.
Smith, Edward James, M. D. on the irritability of vegetables, p. 158.
Spark, electric, decompoſes hepatic air, and leaves a quantity of inflammable air equal in bulk to the hepatic air very nearly, p. 385.
Springs, in Jamaica, p. 60.
Stomach of a cuckoo, ſubſtances found in, p. 235.
Survivorſhips, on the probabilities of, between two perſons of any given ages, and the method of determining the values of reverſions depending on thoſe ſurvivorſhips, p. 331.

T.

Tables.

A table of the mean heat of every month for ten years in London from 1763 to 1772 incluſively, p. 66.

Tables, containing obſervations on the wind and weather, p. 113—120.

Table, containing the reſult of Mr. M'Nab's experiments on the nitrous acid, p. 169.

Meteorological Journal for the year 1787, kept at the apartments of the Royal Society, for January, p. 192, 193. February, p. 194, 195. March, p. 196, 197. April, p. 198, 199. May, p. 200, 201. June, p. 202, 203. July, p. 204, 205. Auguſt, p. 206, 207. September, p. 208, 209. October, p. 210, 211. November, p. 212, 213. December, p. 214, 215. Greateſt, leaſt, and mean heights of the thermometer without and within, and of the barometer; alſo obſervations of the quantities of rain, during the foregoing months, p. 216.

Table of octaves and fifths, p. 246.

Table, ſhewing the probabilities of ſurvivorſhip between two perſons of all ages, whoſe common difference of age is not leſs than ten years, p. 337.

Table, ſhewing the value of 100*l.* computed at 3*l.* per cent. by the Northampton Table of obſervations payable on the death of the younger perſon, if the elder ſurvives him, p. 344.

Table, ſhewing the value of 100*l.* computed at 5*l.* per cent. by the Northampton

Table, payable at the death of the younger perſon, if the elder ſurvives him, p. 345.

Table, ſhewing the value of 100*l.* payable on the death of the younger perſon, if the elder ſurvives him, according to the Sweden Table of Obſervations; calculated at 4*l.* per cent. p. 345.

Table, relating to the Georgian Planet, p. 371.

Of the annual growth of Oak, Aſh, and Elm trees, p. 410, 411.

Table of the number of days in two ſucceſſive Mahometan years (623, 624), p. 421.

Table exhibiting the correſpondence of the years of the Hejerà with thoſe of the Chriſtian era, p. 428—431.

Table containing the compariſon of the commencement of thirty ſucceſſive Mahometan years, with the ſame number of Gregorian years according to the diviſion of the cycle, and appearance of the new moon, p. 432.

Tartar, ſalt of, its effect on the freezing point of water, p. 307.

Thermometer, ſtate of it at London throughout the year 1787, p. 192—216. See *Barometer*.

Titlark, frequently ſelected by the cuckoo to take charge of its young one, p. 223.

Trees, ſome account of the annual growth of, p. 408.

V.

Vegetables, irritability of, ſome obſervations on, p. 158.

Viſcera, account of a remarkable tranſpoſition of the, p. 350.

Vitriol, *Green*, the effect of, in lowering the freezing point of water, p. 292.

Vitriol, *White*, its effect on the ſame, p. 293.

Vitriolic acid, its effect upon the freezing point of water, p. 305.

W.

Walker, Richard, on the production of artificial cold, p. 395.

Waring, Edward, M. D. on centripetal forces, p. 67. On ſome properties of the ſum of the diviſors of numbers, p. 388.

Water, boiled, freezes ſooner than unboiled, p. 128. Oil ſpread over the ſurface of water prevents it from freezing, when other water, expoſed in a ſimilar manner, has had a cruſt of ice formed upon it, p. 136. Water in freezing undergoes a conſiderable expanſion, p. 142. Obſervations and experiments on the compoſition of water, p. 147, 313. Water conſiſts of two kinds of air, dephlogiſticated and inflammable, p. 147. Experiments on the effect of various ſubſtances in lowering the point of congelation

congelation in water, p. 277. When pure water is cooled below its freezing point the leaſt particle of ice or ſnow brought in contact with it cauſes an inſtant congelation, p. 283.

Water-wagtail's neſt choſen by a cuckoo to lay her egg in, p. 223.

Wine, Spirit of, its effect on the freezing point of water, p. 309.

Withering, Dr. letter to Dr. Prieſtley, p. 319. See *Letter*.

FROM THE PRESS OF JOHN NICHOLS.

ERRATA.

Page 367. lines 15. and 16. *read* the numbers as they are printed in the table, p. 371.

P. 373. l. 11. and 12. *read* Ec 89° 27′ 56″,1 . Es 30° 4′ 35″,8; which will, of courſe, occaſion an alteration in the figures, p. 373. l. 14. 26.; p. 374. l. 16. 20, 21. 24. 27. 29.; and p. 375. l. 2. 4. 7, 8; ſo that the reſult, on p. 377. l. 25. will ſtand thus: $\left\{\begin{matrix} 91° & 1' & 32'',2 \\ 89 & 48 & 27 & ,5 \end{matrix}\right\}$.

P. 375. l. 11. after $\left\{\begin{matrix} \text{Eaſt} \\ \text{Weſt} \end{matrix}\right\}$, add the words, "of the pole of the ecliptic,"

P. 378. l. 17. *for* 18 feet 8 inches *read*, 15 feet $3\frac{1}{4}$ inches.